QUARTERLY JOURNAL

OF THE

ROYAL METEOROLOGICAL SOCIETY

Editor : P. A. SHEPPARD

Vol. 78 APRIL 1952 No. 336

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TO HER MOST EXCELLENT MAJESTY ELIZABETH THE SECOND

The Humble Address of the President, Council and Fellows of the Royal Meteorological Society

May it please Your Majesty -

We, Your Majesty's most dutiful and loyal subjects, the President, Council and Fellows of the Royal Meteorological Society, desire to offer our sincere and heartfelt sympathy to Your Majesty, to Her Majesty Queen Elizabeth the Queen Mother, and to members of the Royal Family on the grievous loss which all have sustained in the death of Your Royal Father, His late Majesty KING GEORGE VI.

Our Society's sense of loss is the deeper in that His late Majesty was graciously pleased to accord to us His esteemed Patronage throughout His Reign.

We desire, at the same time, to be permitted to express our thankfulness that the Crown has descended to a Princess who, in common with Her husband, His Royal Highness the Duke of Edinburgh, has displayed the same keen interest in the advancement of Science as was shown by Her Revered Father.

Given under the Seal of the Society, this Twentieth day of February, in the year of Our Lord One thousand nine hundred and fifty two.

President

Secretaries

Facsimile copy of loyal address of condolence from the President, Council and Fellows of the Royal Meteorological Society to Her Majesty Queen Elizabeth II on the occasion of the death of their Patron, His late Majesty King George VI.

QUARTERLY JOURNAL

OF THE

ROYAL METEOROLOGICAL SOCIETY

Vol. 78 APRIL 1952 No 336

EDITORIAL

The content is more than the form. True of the scientific paper as of the didactic or more general essay, yet not so true that we may not often be tempted to the contrary view that content and form are one, even in a scientific paper. These thoughts arise as we receive from the press the second and revised edition of the Society's *Preparation of Papers for the Quarterly Journal* which we most warmly commend to our readers, or to those of them who may at any time take up their pens or strike their typewriters to compose a paper for the *Quarterly Journal*.

The more impatient may think that little advice is needed in putting such objective material as scientific research on to paper. But if he had struggled with many of the manuscripts received by the Society in order that they should reach the printer, blockmaker and reader in acceptable form he would know otherwise to his cost. In fact the preparation of a paper so that it shall be cogent in style and finished in form for printing calls for little more than sympathetic consideration for others – there are few arbitrary rules of the game and nothing occult in the practice. The Society's pamphlet referred to above has been prepared to assist authors in the composition of their papers and to save expense, often considerable, to the Society. Unfortunately it seems frequently to reach the author, if at all, after his paper is composed and may then, we surmise, be a real source of irritation in exposing his sins of omission and commission – it is so much pleasanter to carry out a task properly and with style the first time than to patch and reconstruct afterwards. *Verb. sap.*

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Tropical meteorology

By C. E. PALMER
Institute of Geophysics, University of California
(Formerly of the New Zealand Meteorological Service)

1. Introduction

Advances in meteorological knowledge come about in several ways : as the result of shrewd observation of local wind and weather, by development in instrument design and construction, through the mathematical formulation of deep insights into the physical transformations of the atmosphere, by the distillation of experience with many synoptic maps, and through the statistical generalizations of climatology. No doubt we should like the empirical sources of progress to be geographically unrestricted. We should like to feel that the most general statements of our science rest upon observations uniformly dense with respect to latitude and longitude and that they apply, with known limitations, to atmospheric events over any part of the globe. But meteorological observations are distributed in obedience to political, social and economic demands and not to scientific prescription. Only a small portion of the troposphere is explored from day to day by our instruments. From this fragment we have to reconstruct in synoptic study, in theoretical deduction or in climatology the behaviour of the whole.

Occasionally there are accidents : military necessities suddenly extend the reach of observation ; routine observations can be made more accurate by instrumental improvements. In either event the results usually astonish us. The new regions thus opened up for exploration only vaguely resemble those pre-conceived by the theoreticians. It is not only that the griffins and basilisks described by the philosophers are absent ; it seems that the country is occupied by creatures of which they have never dreamt.

With respect to one region, at least, the upheavals of the past twelve years have caused such an accident. The tropics have been opened up for meteorological exploration. It is true that huge areas are still almost completely unknown : the eastern Pacific, equatorial America, the South Indian and South Atlantic Oceans. But there now exists a volume of data referring to the western Pacific, to the Caribbean and to Africa that is vastly increased over the previous information which was, in comparison, almost static for three hundred years. Yet tropical data for the years of World War II, valuable as they are, hardly attain, in density or accuracy, to the normal observing standards of North America and Europe. Excellent authors have studied them, and it seems that they have abstracted most of the empirical results that the wartime network is likely to yield.

In one region alone, and that for very brief periods, have the quality and density of the data reached the required standards – the Marshall Islands (see Fig. 1). Here, as a result of nuclear weapon tests, three short periods, in 1946, 1948 and 1951, yielded good meteorological observations. This article is written with the conviction that in that region and at those times the atmosphere provided data which were adequate to form and check synoptic hypotheses ; the first part develops these hypotheses, the second investigates the possible ways in which they might apply to tropical regions other than the Pacific.

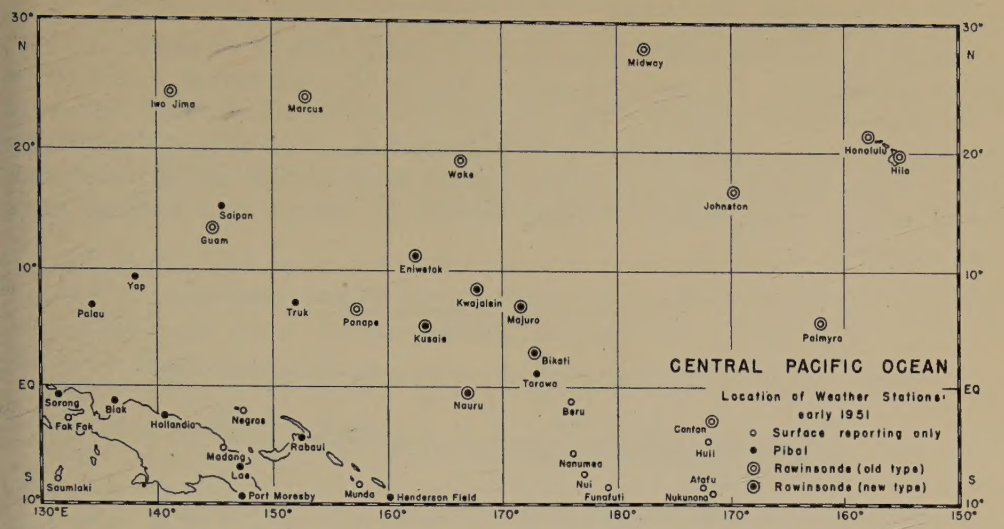


Figure 1. Weather observing stations in the Central Pacific.

2. Development of the hypotheses

(*a*) *The equatorial wave – an example*

A previous work (Palmer 1951b), which was in a sense the introduction to this, attempted to resolve the historical confusions of tropical meteorology. The result of the survey may be briefly stated : while weak temperature and pressure gradients exist in the tropics and may in limited regions (e.g., temperature gradients in equatorial Africa, pressure gradients in tropical cyclones) play a major role in synoptic analysis and in physical interpretation, the field of motion alone displays the most easily observed and the most consistent variations in time and space ; further, the distribution of derived quantities, the vertical component of vorticity and the horizontal velocity divergence, is such as to suggest that the great uniform basic currents, the trades and equatorial easterlies, are subject to wave-like perturbations. The waves, remarkably uniform in wave length and speed of propagation, no matter whether they are found over the Pacific or the Atlantic, vary widely among themselves with respect to stability. Unstable waves are transformed into vortical circulations, the more intense of which become evident during analysis of the field of pressure as easterly ' waves ' or tropical depressions. By the end of World War II these conclusions had been reached by one school of tropical meteorologists (Dunn 1940, Riehl 1943, Simpson 1945). The Marshall Islands data not only confirm the work of the authors quoted but amplify and extend it. The best introduction to the methods of analysis and the synoptic theories of the school, therefore, is a discussion of a comparatively simple synoptic situation in the central Pacific.

In any synoptic work there is always one class of maps the study of which leads to a definitive analysis – the one to which pre-eminence is given either because it refers to the greatest number of observations or because the changes of the variables in space and time that it displays are large, consistent and easily understood. In spite of the greatly increased volume of information about the upper air that now flows into weather stations in middle and high latitudes, the definitive analysis is still that

of the surface-pressure map. For reasons which will become clear, there is a great advantage in departing from this convention in the analysis of equatorial and sub-equatorial weather. The definitive analysis for the Marshall Islands is that of the wind field at 1,500 or 2,000 ft. The former level is almost always below that of the cloud bases, it is accessible to pilot-balloon observation as well as to the rawinsonde and it is the prescribed level at which weather-reconnaissance aircraft make their lowest surveys. Here flight-level winds obtained by multiple-drift methods attain high accuracy.

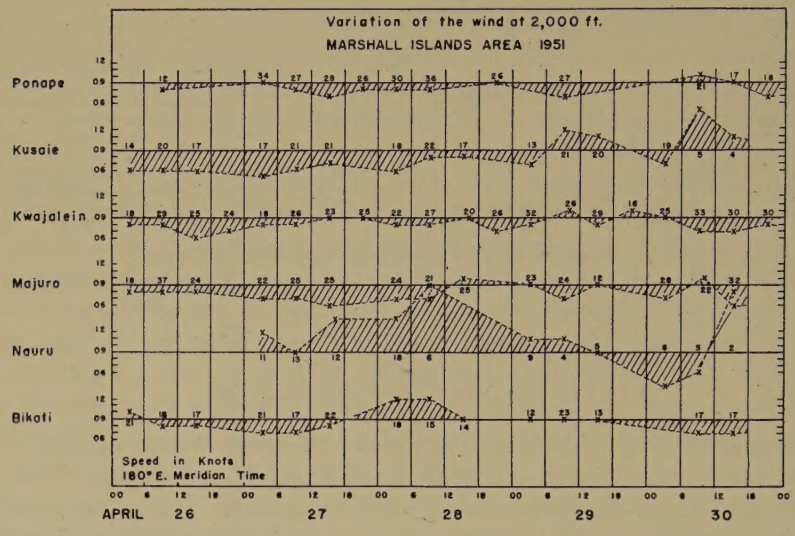


Figure 2. Variation with time of the wind at 2,000 ft in the Central Pacific ; 26 April to 30 April 1951. Ordinate direction in 10's of degrees. Speeds entered on 90° line.

Fig. 2 shows the variation with time of the wind at 2,000 ft from 26 April to 30 April 1951 at 6 stations in the central Pacific. The location of the stations can be checked on Fig. 1. The curves representing the variation in wind direction with time are shaded in a manner that emphasizes the changes with respect to 90° — the east wind. It should be remarked that the observations were made at all these stations by means of rawinsondes and that the receiving equipment at Bikati, Nauru, Majuro, Kwajalein and Kusaie was of a new self-tracking and automatically-recording type that provides wind observations of a high order of accuracy.

In Fig. 2 the curve for Bikati is the first item of interest. On the 26th and 27th the wind was north of east and fluctuating in speed about 20 kt. Late on the 27th it began to move south of east, attaining 120° by the morning of the 28th. The wind speed still did not vary significantly from 20 kt. By the afternoon of the 29th the direction was changing toward east-northeast again. It seems that, during the period of interest, a slight wave-like disturbance was passing over the station. At Nauru the wind was already south of east at the beginning of the record ; nevertheless the amplitude of its southerly departure from east increased until midday on the 28th, after which there was a gradual turning back to north east on the 30th. The disturbance clearly had its greatest amplitude at Nauru. Moreover the winds were generally lighter at that station. The southerly late on the 30th does not directly appertain

to the disturbance in which we are interested. It ought to be pointed out, however, that since the wind was very light, 2 kt, its great departure from east is probably not significant.

Comparison of the graphs for Bikati with those of the other stations and with the positions of observation in mind, suggests the following :

(a) At Bikati the basic direction of the current which was disturbed was easterly.

(b) At Nauru the basic direction about which the wind oscillated was south of east.

(c) At all other stations the basic current was predominantly north of east.

(d) The oscillation had its greatest amplitude near the equator and was damped toward the north, being almost imperceptible at Kwajalein, Majuro and Ponape, all of which lie north of 5°N.

(e) Stations in the eastern Marshalls showed the wind changes before those in the west, i.e., the disturbance moved from east to west.

(f) In general the speed of the winds increased from the equator northward.

Figs. 3 and 4 show the definitive analysis of the disturbance in the field of motion at 1,500 ft, at 09 hr 180th east meridian time (hereafter referred to as Local Standard Time) on 28 and 29 April 1951. The analysis consists of two sets of lines : the streamlines, to which everywhere the wind vectors were tangent at the instant of observation, and the *isovels* (also by purists called the *isotachs*) which are lines joining points of equal wind speed. The method by which these maps have been analysed, the isogon method, is fully described in the literature (Sandström 1909, V. Bjerknes 1911). The aircraft observations were not, of course, all made at the same time. Allowance has been made for the movement of the perturbation from east to west during the reconaissance and the individual observations have accordingly been referred to positions upstream or downstream. All aircraft winds were observed by multiple-drift methods ; they were not, therefore, the usual inaccurate navigation winds derived by dead reckoning or loran fixes.

The maps reveal a feature not obvious from Fig. 2 : the presence of a small vortex at 1.8°N., 156.4°E. on the 28th. The location of this disturbance and the details of the streamline pattern surrounding it are of a lower order of accuracy than the remainder of the analysis. Its position is based upon aircraft observations made by typhoon-reconnaissance aircraft from Guam ; apart from the pilot-balloon observations at Rabaul, which are shown in Table 1, the land stations gave no hint of its existence.

TABLE I. UPPER WINDS AT RABAUL, 10 LST, 26 TO 30 APRIL 1951. DIRECTION IS GIVEN FIRST, IN DEGREES. SPEED IN KT.

	Surface	1,000 ft	2,000 ft	3,000 ft	5,000 ft	7,000 ft	10,000 ft
April 26	220 1	320 4	300 10	290 10	290 14	330 7	
27		290 5	310 7		310 3	260 4	300 4
28		300 11	290 14	300 16	270 4	220 1	
29			150 3	000 0	150 1		
30				missing			

The position of the vortex is interpolated on the map for the 29th and the exact course of the streamlines in its vicinity is open to doubt. On the 30th, reconnaissance from Guam again located the centre at 8°N., 141.5°E. It was then more intense

and its intensity steadily increased until it became the typhoon named, in accordance with the practice in the area, ' Iris.' This storm was under observation from 30 April until 12 May. However, it is not the present purpose to study the ' Iris ' vortex which is in its earliest stage on Fig. 3. The wave that followed it is much better authenticated by the observations from the fixed stations. The maps confirm and amplify the conclusions (a) to (f) previously suggested, and which are made explicit on the figures by the words 'positive axis' and 'negative axis.' The lines so marked follow the course of the 90° isogon, i.e., the line joining the points at which the wind is a true easterly. In equatorial synoptic meteorology it is confusing to speak, as is the custom in high latitudes, of ' trough ' and 'wedge' lines. The equator marks no break in the continuity of air movement and troughs and wedges change their names as they cross it. It is therefore appropriate to call the axis of a wave along which the wind changes from a direction north of east to south of east the ' positive ' axis of the wave, while the line of opposite change from south-east to north-east may be named the ' negative ' axis. Since the wave is damped toward the north and since the direction of the wind at 10°N. is everywhere in the area northeasterly, the positive and negative axes are necessarily continuous. In fact, the line consisting of the continuous positive and negative axes itself has a wave-like form.

At this point it is as well to enquire whether the disturbance that appears on the maps of Figs. 3 and 4 is as complicated as it seems at first sight, whether, in fact,

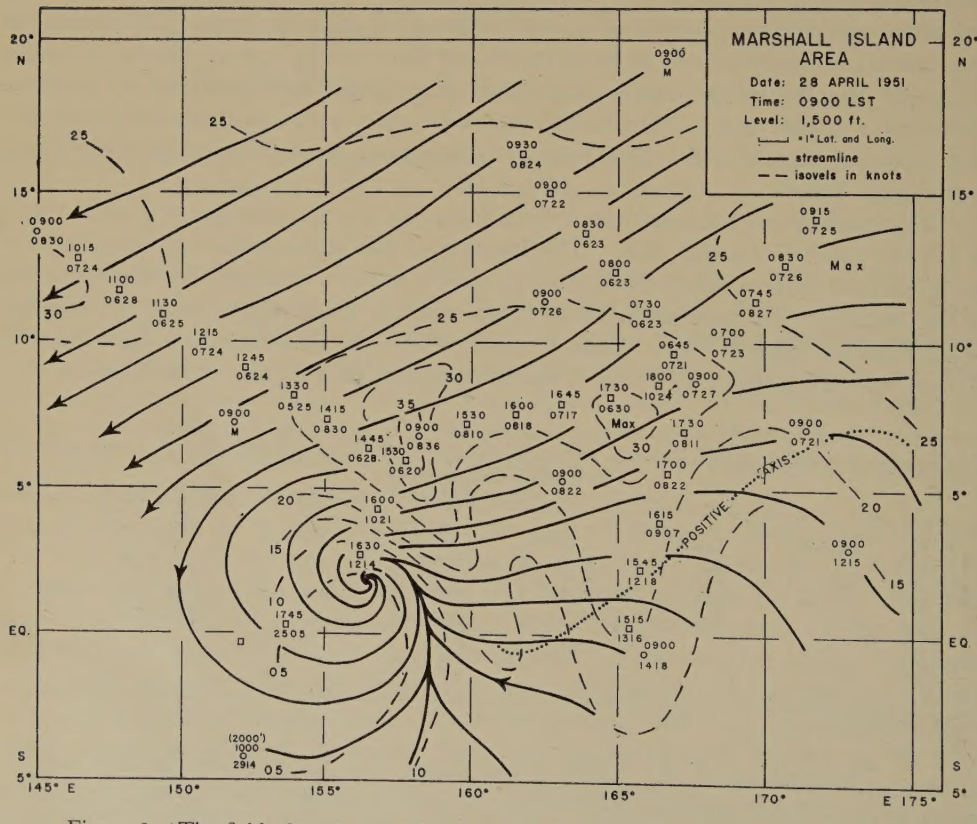


Figure 3. The field of motion at 1,500 ft, Marshall Island areas ; 09 LST, 28 April 1951.

analysis can effect some simplification. The axes or defining lines of the wave have more or less arbitrarily been fixed as the positions of the 90° isogons, chiefly because theoretical meteorology has to a large degree dealt with basic undisturbed currents which are entirely zonal ; we naturally think of the general and most basic currents as the tropical easterlies or the high-latitude westerlies. However, there is in synoptic meteorology no necessity for adhering to this concept. In synoptic work the natural division of any given field of motion might well be into that part which does not change with time, or which, on the time scales contemplated, changes very slowly with time, and that part which changes rapidly, but not so rapidly nor on such a small scale as to belong to the turbulent disturbances of the air movement. The field that does not change with time is here called the basic current and the changing component is called the disturbance or perturbation. There are no implications involving the relative magnitude of the components, no question involving the theory of small perturbations.

Points (a), (b) and (c), above can be examined with these motions in mind. They can be interpreted as follows : throughout the passage of the disturbance, the basic current was east-north-east in direction at Majuro, Kwajalein, Kusaie, Eniwetok and Wake. It was easterly at Bikati, south-east at Nauru. With the old climatological model of an ' equatorial front ' or intertropical convergence line in mind, and remembering that the winds increased in speed toward the north, we can reconstruct a qualitatively correct model of the basic current. It is, in fact, very similar to the streamline

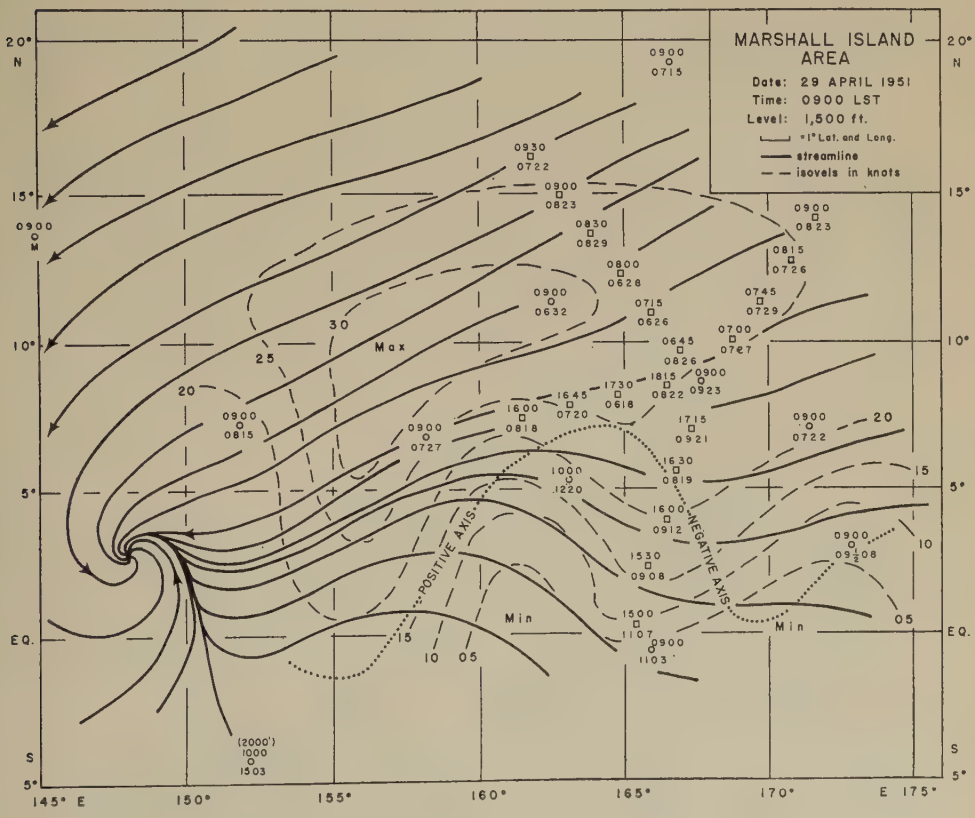
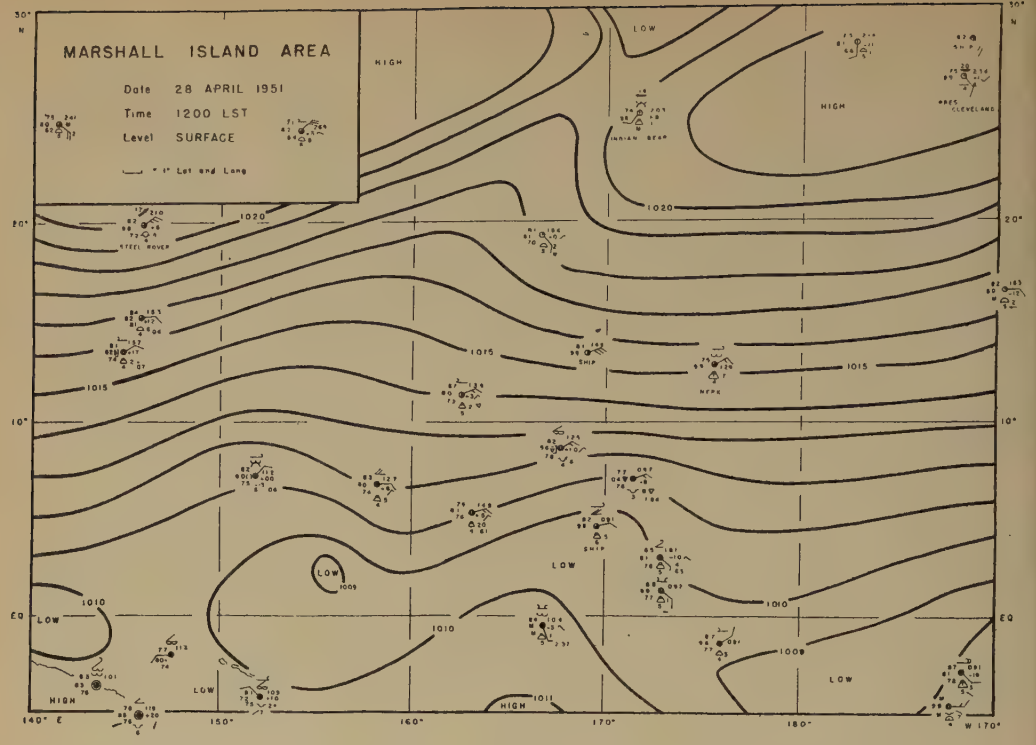


Figure 4. The field of motion at 1,500 ft, Marshall Island area ; 09 LST, 29 April 1951.



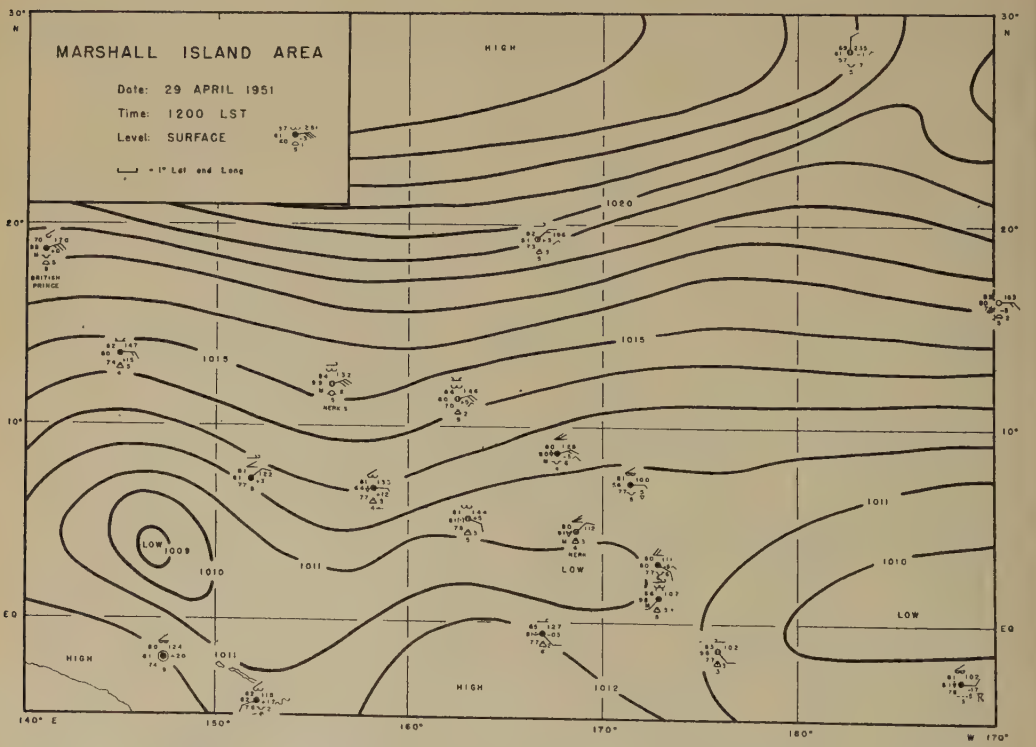


Figure 5 and 6. Surface pressure, Central Pacific. The station pressures are given as transmitted. Kusaie is about 4 mb too high owing to faulty height correction. Majuro also has a systematic error; it is almost 2 mb too low. Most ships give unreliable aneroid readings. Figure 5. 12 LST, 28 April 1951. Figure 6. 12 LST, 29 April 1951.

pattern at the surface shown on mean maps for the summer months in the Marshall Islands. The 'axes' of the perturbation now take on a different form. They would correspond to the lines joining the places at which the actual wind was equal to that in the basic current. The whole disturbance, on this view, was a moving wave approximating to a harmonic perturbation of the basic current, but with an extreme lateral damping. The amplitude of the disturbed streamlines, therefore, decreased rapidly from the equator toward the north and (as other evidence shows) toward the south in the southern hemisphere.

Two other properties of the wave may be derived from Figs. 3 and 4. The wave length of the disturbance was roughly 15° of longitude and its speed of propagation 11 kt toward the west. These numbers are in striking agreement with the values for the wave length and speed of propagation of easterly waves in the Caribbean and the North Atlantic given by Riehl (1943) and others. In fact, the great majority of west-moving waves on synoptic maps of both the Atlantic and the Pacific move with a speed between 10 and 15 kt; this is also the most frequent speed of hurricanes and typhoons before recurvature. The second feature of the wave, characteristic of the class of disturbances to which it belongs, is the amplitude of the isovels; these have in the majority of cases a greater amplitude than the streamlines.

From the definitive analysis we can pass directly to the surface-pressure analysis shown on Figs. 5 and 6. The gradients of pressure close to the equator are usually so weak, the spacing of the stations so open, the diurnal pressure variation so large and the index and other errors of the barometers so uncertain, that without the previous analysis of the 1,500 ft wind field, many qualitatively different analytic solutions are possible. Knowing the nature of the wave in the field of motion, however, the analyst is on the alert to discover very small but significant wave-like disturbances in the pressure field. Figs. 5 and 6 show that the equatorial low-pressure trough corresponded roughly in position with the line of convergence in the wind field at 1,500 ft. Superimposed upon this trough was a pressure wave of small amplitude; as might be expected from the fact that the isovels had a larger amplitude than the streamlines, the isobaric disturbance was of smaller amplitude than that in the streamline field.

The properties of the class of waves to which the example under study belongs may be made formal as a model. The model is built up step by step in Figs. 7 – 13.

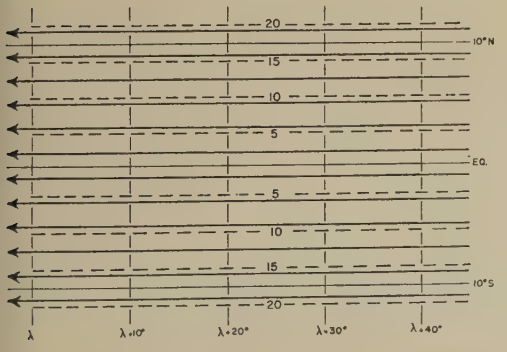


Figure 7. Idealized basic current without horizontal velocity divergence; Central Pacific.

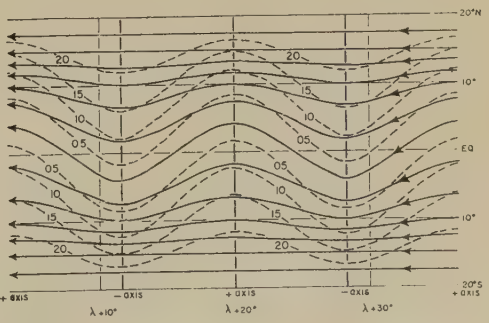


Figure 8. Basic current of Fig. 7 disturbed by an equatorial wave.

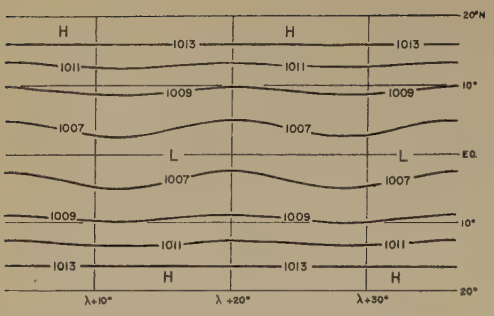


Figure 9. The pressure field corresponding to Fig. 8.

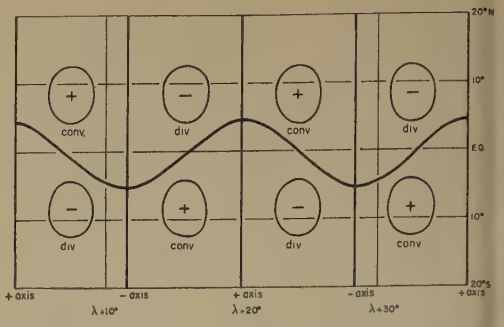


Figure 10. Qualitative estimate of the vertical velocity in the lower atmosphere corresponding to Fig. 8.

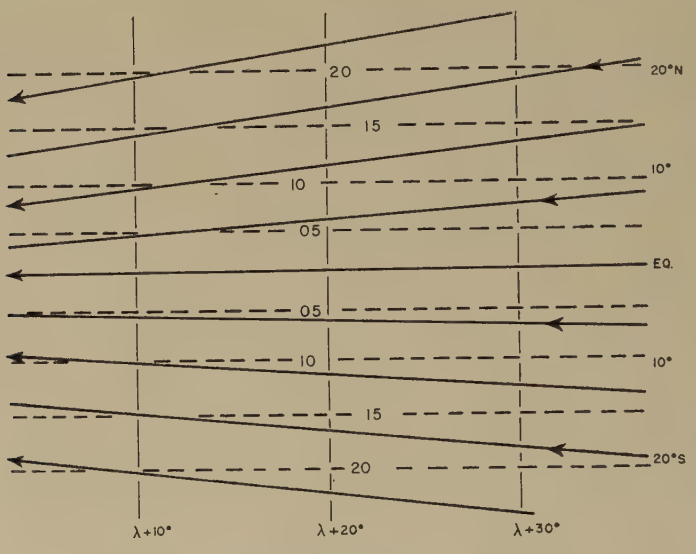


Figure 11. Idealized basic current of Central Pacific with the distribution of convergence, known as the ' equatorial front.'

It should be emphasized that these diagrams represent a synthesis of synoptic experience; their basis is empirical, not theoretical. Fig. 7 is an ideal, showing a basic equatorial current without horizontal velocity divergence, symmetrical about the equator and unchanging in time. A current like this is occasionally encountered in synoptic experience in the central parts of the Pacific, between 160°W. and the 180th meridian, and it resembles some theoretical conceptions of the tropical part of the planetary circulation. Fig. 8 shows the same current disturbed by an equatorial wave of the type we have been considering. The wave is imperceptible at 20° N. and S. ; the amplitude of the streamlines is less than that of the isovels. Fig. 9 shows the corresponding perturbation of the equatorial pressure trough. From Fig. 8 it is possible to estimate the zeros, maxima and minima of the horizontal velocity divergence and consequently, with some approximations, those of the vertical motion at 1,500 ft – this is represented on Fig. 10.

Figs. 11, 12 and 13 represent a situation more closely resembling the majority of equatorial disturbances. The basic current is complicated by an ' equatorial front,'

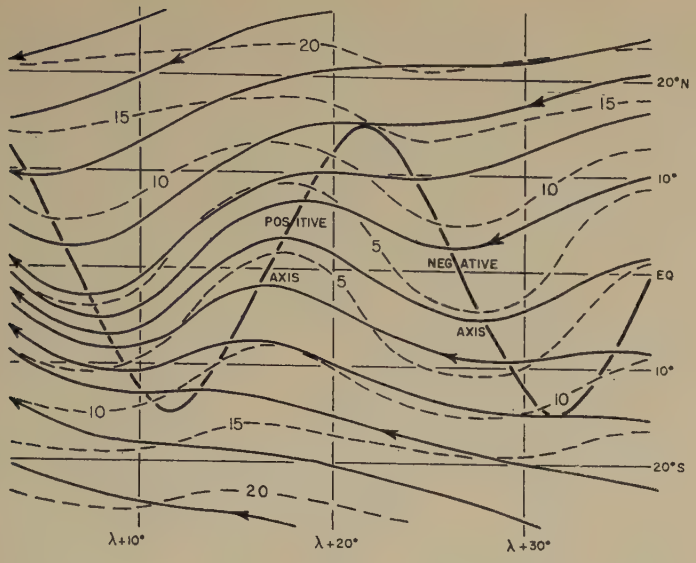


Figure 12. The current shown in Figure 11 disturbed by an equatorial wave.

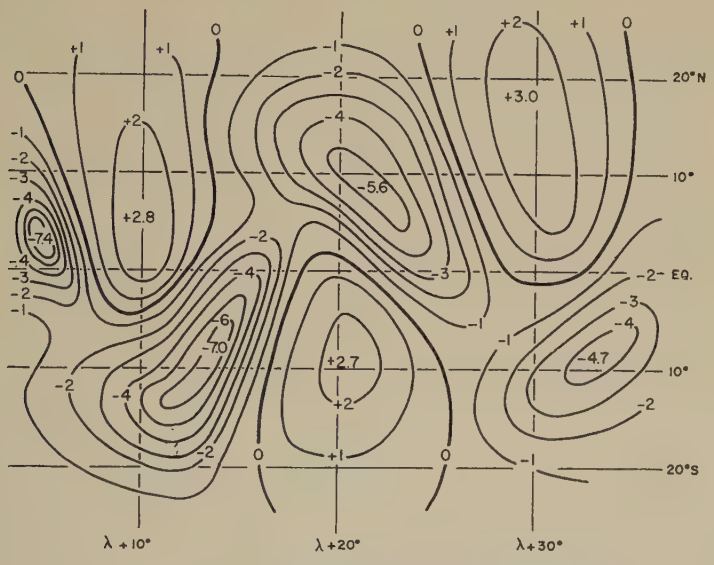


Figure 13. The distribution of horizontal velocity divergence, computed from Fig. 12 (unit = 10^{-8} sec^{-1}).

a line of convergence, in the ideal case coinciding with the equator. The whole lower part of the basic current is characterized by a negative horizontal velocity divergence and consequently by positive vertical motion. Fig. 12 displays the same current disturbed by a simple, approximately harmonic perturbation. This model resembles the disturbance on the synoptic maps. There is no sign of the intertropical front in the disturbed motion and the isogon for 90° itself has a sinusoidal form. The streamline disturbance hardly affects stations beyond 15° N. and S. However, the

isovels have a greater amplitude than the streamlines so that the disturbance can be detected as variations in the speed of the north-east and south-east trades even as far as the 20° parallels. It is clear that if a train of such disturbances passed over an area in the neighbourhood of the equator, say for a month, and the resultant winds were computed, the streamline pattern for the mean resultant winds would resemble very closely those showing an equatorial front, for example those drawn by Werenskiold (1923). This point is beautifully illustrated by streamline analyses of the 1,500 ft winds for the month of June 1946, during the atomic weapon testing on Operation 'Crossroads.' A succession of equatorial waves similar to that already illustrated passed across the central Pacific. The maps show no obvious evidence of a continuous equatorial front persisting from day to day : nevertheless, when mean winds are derived from the resultant winds for each 5° intersection, the streamline map shown on Fig. 14 is obtained. This may be compared with Werenskiold's analysis of the surface winds for the same area and for the month of June. Fig. 12 has been drawn freehand, without any attempt to make the perturbation strictly harmonic. The horizontal velocity divergence can be computed by standard methods from such a streamline map and Fig. 13 is the result. The values of the divergence agree very well with those obtained from actual cases ; the patterns, also, are realistic. Although the wave is of small amplitude, the positively divergent regions associated with it are sufficiently large to overcome the convergence in the basic current. However, the convergent areas of the perturbation are reinforced by that of the basic current. The negative areas *in toto*, then, form an undulating band crossing and recrossing the equator ; there is a rough correspondence between this band and the band of heavy cloud and precipitation, formerly analysed as 'equatorial front weather,' in similar situations.

Such then are the properties of the small stable equatorial wave in the lowest layers of the atmosphere. Its vertical structure must now be studied. In this context the question of 'air masses' can be considered. The uniformity of the surface temperature and dewpoint fields is sufficiently obvious from Figs. 5 and 6.

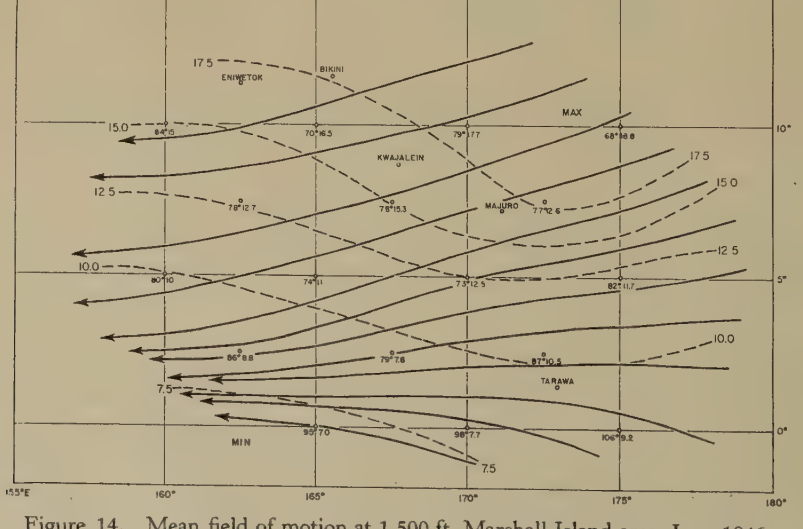


Figure 14. Mean field of motion at 1,500 ft, Marshall Island area, June 1946.

The basic current, it will be recalled, has been defined as that part of the motion which is independent of time. If, and only if, the integral over some specified time interval of the perturbation of the motion at each station is zero, will the mean velocity taken over the same interval be equal to this basic current. Without entering into details, it can be said that the condition is satisfied to a high degree of approximation in the situation under study. By computing the mean resultant wind at each level for each station we may therefore arrive at an approximation to the basic current for the period 26 to 30 April 1951. Fig. 15 shows the zonal component of the basic current derived in this manner from Marshall Islands stations distributed about 167°E. and from the equator to 20°N. It must again be emphasized that the basic current revealed by the diagrams is that which prevailed in the Marshall Islands and their

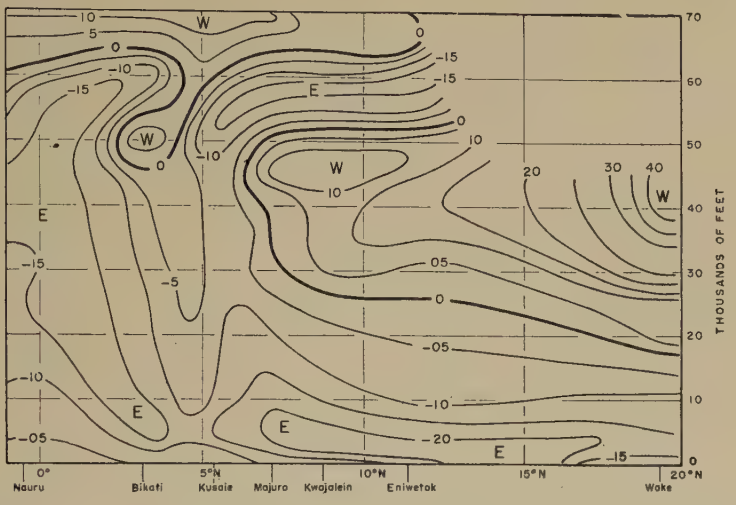


Figure 15. Mean zonal component of the wind near 167°E. ; 26 April to 30 April 1951.

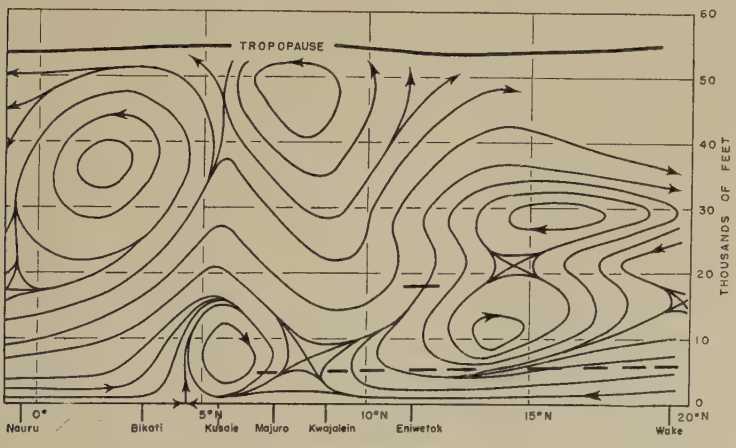


Figure 16. Mean streamlines in the meridional plane along 167°E., 26 April to 30 April 1951. The meridional and vertical components are combined but isovels are omitted, as the vertical scale is much exaggerated. Heavy quasi-horizontal lines represent most frequent positions of inversions during the period.

environs during the five days at the end of April 1951. It is *not* legitimate to generalize from the diagrams to other tropical Pacific longitudes or to other periods in the same area. Further, the method of obtaining the current can only be used when it is clear from a preliminary study of the observations that the assumptions upon which it is based are valid. As pointed out in a previous discussion of this topic (Palmer 1951b), failure to observe these conditions can lead to a fallacious ' general circulation ' in low latitudes.

In spite of the fact that the example is chosen from April, which is regarded as a ' dry-season ' month in the area, Fig. 15 displays a distribution of the zonal component commonly accepted by modern tropical meteorologists as typical of the summer or ' wet season.' The equatorial easterlies were unusually deep (see Riehl 1943). Near 4°N. for example, the easterlies extended from the surface at least to 45,000 ft and probably, over a narrow latitude belt, to the tropopause. The zero isopleth, often called the *base of the westerlies*, was also unusually high, even at Wake Island (approximately 19°). The average height of the isopleth at Wake during July 1946, a typical wet-season month, was 14,000 ft, while during this period it was 18,000 ft. Space will not permit the discussion of all the features of interest on this section, particularly the course of the isopleths above 45,000 ft. It is sufficient to point out that the deep easterly current is now regarded by tropical meteorologists as the basic current most apt to be perturbed by easterly waves and equatorial waves – in general waves that move toward the west with a speed between 10 and 15 kt and with a wave length of approximately 15° longitude. It should also be remarked that conditions approximating to those idealized in Fig. 11 obtained, so far as the zonal component was concerned, between 3,000 and 20,000 ft – with the proviso that north of 4·5°N. the speed maximum moved toward the meteorological equator with height, and away from it to the south. Above 20,000 ft the horizonal shear was quite different from that shown in Fig. 11.

To complete the survey of the basic current, Fig. 16 shows both the meridional and the vertical components of the wind, combined in a streamline map for the meridional plane. The meridional component has been computed directly as the five-day mean (26 to 30 April). The vertical component has been computed from the continuity equation under the assumption of negligible local change of density throughout the period. The method of computation, in fact, is that called by Panofsky the kinematic method (1946) ; it gives reasonable results with winds obtained from the newest type of rawinsonde equipment. In passing, it should be mentioned that the effect of the variation of the zonal component of the wind along the latitude circles was, in this particular case, negligibly small, and it has been completely omitted in the computation. With all these assumptions and errors involved, the magnitudes of the vertical components are, of course, of an order of accuracy much lower than that of the meridional components ; nevertheless, experience with the type of data contemplated here suggests that in this section the sign of the vertical motions is reliable. Certainly the order of magnitude discovered is such as we might expect on other grounds. The values range from the extremes +15 to −20 cm/sec, the greater part of the field being between +5 and −5 cm/sec. The maximum magnitude of the meridional component is about 5 kt (2 to 3 m/sec). In some ways Fig. 16 is of greater interest than the section showing the zonal component. It has been remarked in the *Handbook of Meteorology* (1945) that it is difficult to find evidence of a *return current* or anti-trade at high levels in low latitudes. The reason, perhaps, is not hard to find. Outside the region between 3°N. and 13°N., where on this section complexities exist, the return currents on both sides of the

meteorological equator are found in the high troposphere – above 30,000 ft – and these levels have only recently been consistently explored near the equator. It must not be supposed, however, that the simple trade-anti-trade model of the general circulation, the so-called direct cell, is completely validated by these data. Fig. 16 shows that conditions are more complex than the climatological school imagined. The trades of the two hemispheres, which in this example converge at 4·5°N., do so along a boundary that extends only to 15,000 ft. The southern hemisphere southeasterlies then split into two branches, one of which continues to ascend and ultimately returns toward the south, while the other moves onward into the northern hemisphere, ultimately joining the northern hemisphere return current whose main ascending body lies between 11°N. and 13°N. It should be explained that the break-through of the southerlies in the layer between 10,000 and 30,000 ft consistently occurs in the longitudes of the Marshall Islands. It is not a transitory feature confined to short periods or certain months, but appears to be frequent during all months of the year.

In modern meteorology, the climatological concept of trades and anti-trades, symmetrical about the earth's axis, has been replaced by J. Bjerknes's model of the sub-tropical anticyclone (1935). We may apply the model to the situation under discussion. The Marshall Islands lie in the southwest part of the great quasi-permanent anticyclonic cell of the North Pacific, so that, while we expect the trade winds to be northeasterly in direction in the lowest layers, we may also expect that, aloft, winds with a southerly component would prevail. The axis of the anticyclone slopes with height from east to west and toward the equator ; at a fairly low level, therefore, the circulation over the Marshalls is that characteristic of the southwestern quadrant of the anticyclonic eddy. The explanation of the break-through of the southern hemisphere southerlies that appear on Fig. 16 is consequently simple : the main stream above 10,000 ft consists not only of air from the equatorial easterlies turning poleward over the islands as part of the eastern North Pacific cell ; it also consists of air from the southern hemisphere that originally was part of the southeast trade-wind system, and which has crossed both the meteorological and the geographical equators.

The picture which this analysis of the basic current suggests then is in agreement both with the climatological and with J. Bjerknes's view of the general circulation. The direct cell circulation is present but is complicated by the existence of the standing waves we identify as the semi-permanent anticyclones. This view may be checked by moving downstream to a station the circulation above which lies out of reach of the North Pacific cell, at least below 30,000 ft but within the southeast quadrant of the following, less permanent anticyclone. At such a station we should expect northerly components to dominate the circulation, with little or no sign of the break-through southerlies of the southern hemisphere ; as a hypothesis, it might be suggested that the profile would be the mirror image of that taken south of the equatorial trough on Fig. 16, say at Nauru. This would imply that northerly components would prevail up to 30,000 or 35,000 ft with a southerly return current between the upper level and the tropopause. Fig. 17 shows the profile for such a station, Guam, the means being taken for the period 26 to 30 April 1951 as before ; it is compared with Eniwetok, a station in the Marshalls at a comparable latitude, and with Nauru. The absence of the break-through southerlies at Guam is striking. Even more striking is the fact that the Guam section approximates to the mirror image of that at Nauru. Even the magnitude of the maxima and minima above 10,000 ft are, within the limits of error, equal. On the other hand the heights at which these maxima and minima are found

differ by as much as 10,000 ft, a result that perhaps could have been expected, since Nauru is so much closer than Guam to the meteorological equator.

Given the vertical structure of the basic current, we may investigate that of the disturbance, by determining the extent of the variation at the standard levels of the actual wind about the mean. Since the 1,500 ft analysis has already shown that the disturbance was laterally damped and since the basic current approximated to the ideal zonal flow only in and above the region we have called the meteorological equator, the vertical structure of the disturbance should be most clearly shown by the temporal departures of the winds from their mean values at Bikati, situated very close to the line of intertropical convergence. Fig. 18 shows the variation of the zonal and meridional components of the winds at this station about the mean at 5,000 ft and 25,000 ft. At 5,000 ft the wavelike oscillation shown on Fig. 2 can be detected in the meridional component, the wave being approximately in phase with that at 1,500 ft. At 25,000 ft, however, the wave has almost completely reversed phase. More detailed investigation shows that the reversal takes place somewhere in the layer between 15,000 and 20,000 ft, the oscillation being small and difficult to evaluate in this layer, but plainer above. There appears to be a small slope eastward with height in the perturbation above 20,000 ft, but the uncertainties in the method of evaluation, particularly in the assumption that the mean wind represents the basic current, prevent a very definite statement on this point.

The discussion of the vertical structure of the atmosphere during the period of interest is brought into focus by the streamlines at 25,000 ft, shown on Figs. 19 and 20. The extent and position of the anticyclonic circulations hitherto invoked hypothetically to explain the nature of the basic current are plain to see. The wave, marked by the positions of the positive and negative axes at this level, would have passed undetected without the foregoing analysis, since the southerly circulation about the southwest quadrant of the eastern anticyclone so overshadows it.

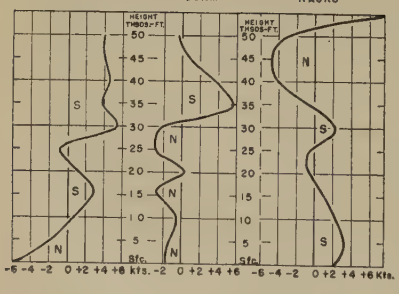


Figure 17. Profiles showing the mean meridional components at Eniwetok, Nauru and Guam ; 26 April to 30 April 1951.

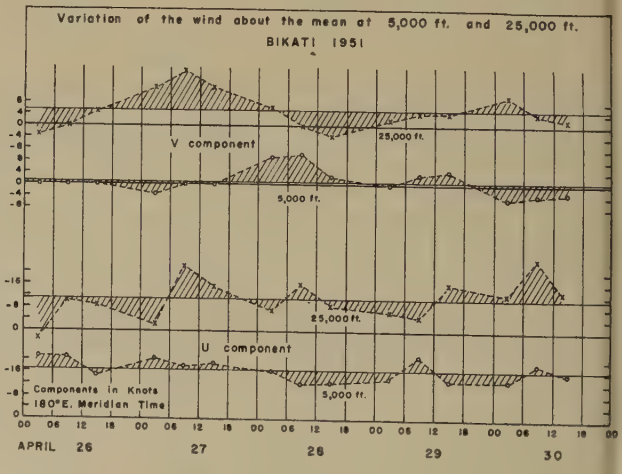
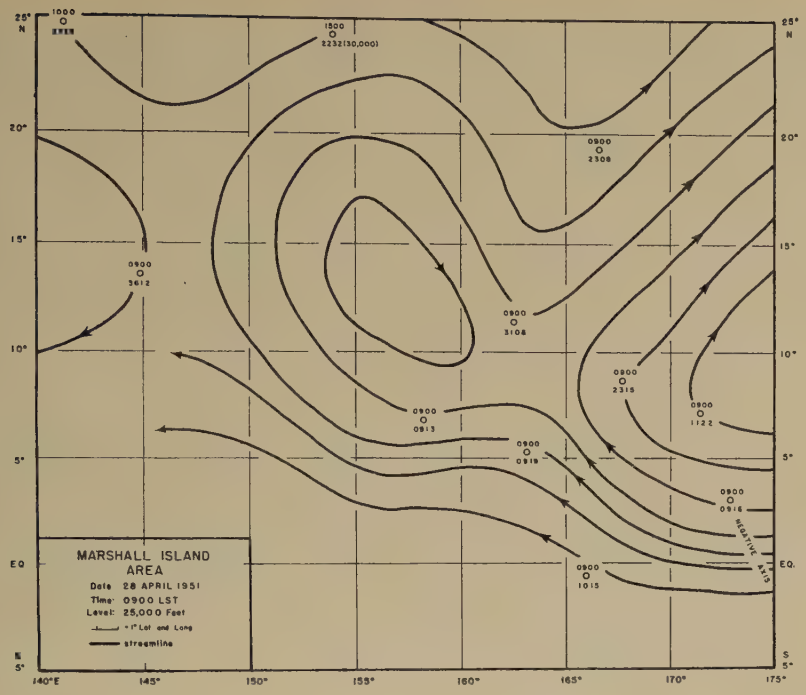
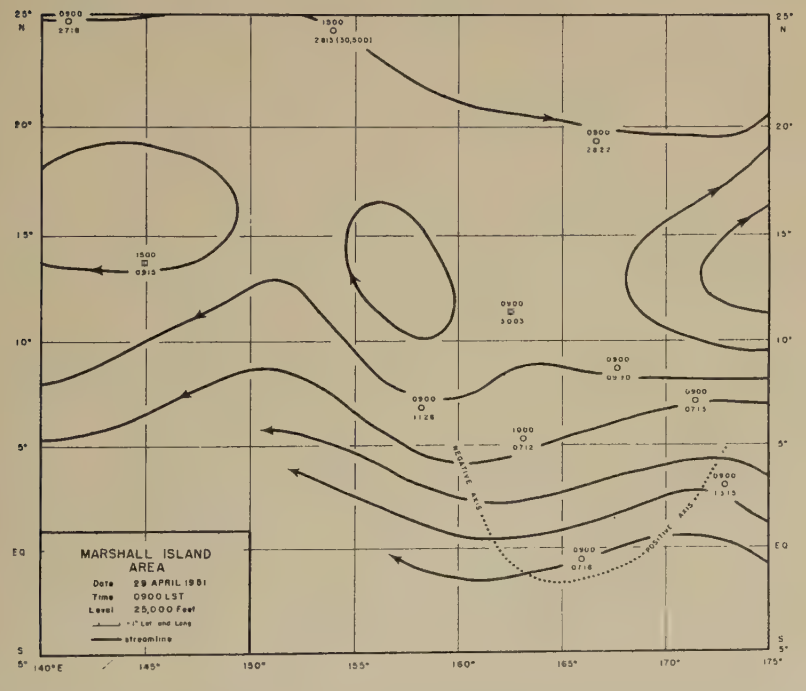


Figure 18. Variation of the zonal and meridional components of the wind at 5,000 ft and 25,000 ft at Bikati ; 26 April to 30 April 1951.





Figures 19 and 20. The streamlines at 25,000 ft, Marshall Island area. As there are no aircraft reports at this level, isovels are omitted as uncertain. Figure 19. 09 LST, 28 April 1951. Figure 20. 09 LST, 29 April 1951.

There remains one question to discuss ; is there any sign of an air-mass difference on either side of the ' equatorial front ' in the basic current ? During the period March to May 1951 the rawinsondes of the Marshall Islands network reached well into the stratosphere three times a day. Eniwetok had poorer flights than those at the other stations but usually managed to reach the tropopause. In consequence, there is a wealth of data on winds, temperatures and humidities on both sides of the equatorial asymptote of convergence. It is impossible to display all this material here ; however, its general effect may be summarized. For the most part the soundings north and south of the meteorological equator and within the zone 15°N.-15°S., when superimposed on the same thermodynamic diagram, coincide from 1,000 mb to 300 mb (and sometimes 200 mb) within the limits of observational error. The differences between stations at the same synoptic hour are smaller than the differences at each individual station between successive soundings attributable to diurnal variation.

Occasionally in the layers between 1,000 mb and 700 mb inversions and isothermal strata show up at some stations and not at others, giving rise to differences between stations of as much as 2°C over limited isobaric slices. But such differences can almost invariably be traced to the passage of the instrument through part of a cumulus congestus or to subsidence. They are as likely to produce differences between neighbouring stations in the same presumptive air mass as between those in presumptive southern hemisphere and northern hemisphere air.

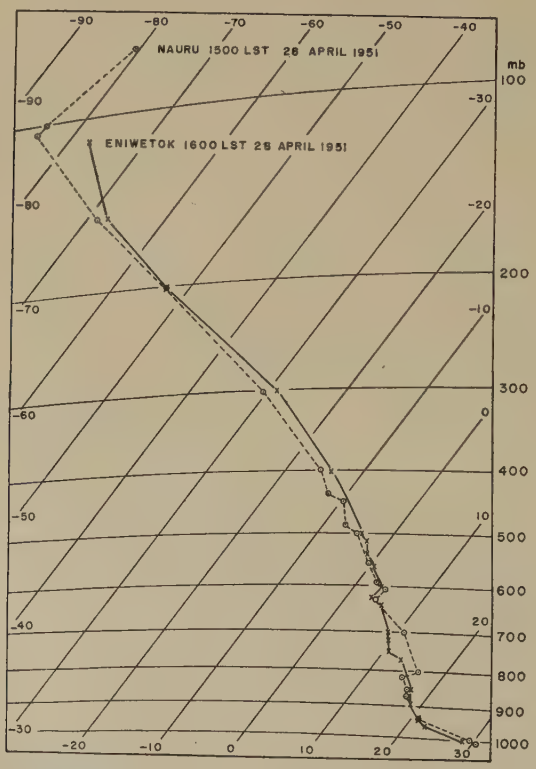


Figure 21. The variation of temperature with pressure at Nauru and Eniwetok, 15 and 16 LST, 28 April 1951.

Fig. 21 provides an example. Eniwetok and Nauru are over 700 mi apart and on a conventional equatorial-front analysis would certainly be considered to lie in different air masses, the former in the northeast, the latter in the southeast trades. The inversion at Nauru is evanescent; Table II shows the temperatures at the two stations at 15 L.S.T. 29 April 1951. An inversion now appears at both stations and its base differs by only 10 mb, undoubtedly within the observational error.

TABLE II. COMPARISON OF TEMPERATURES AT NAURU AND ENIWETOK, 15 LST, 29 APRIL 1951. STATIONS LIE ON EITHER SIDE OF THE 'EQUATORIAL FRONT.' INTERMEDIATE VALUES OF PRESSURE DISPLAY THE ONLY INVERSION PRESENT.

Pressure (mb)	1,000	930	920	900	850	800	700	600	500	400	320	300
Nauru (°C)	29·0	21·5	22·0	21·0	18·0	15·4	9·2	3·0	−4·8	−15·3	−27·0	−30·5
Eniwetok (°C)	29·1	21·1	20·5	21·0	18·1	15·0	9·1	2·8	−4·8	−15·1	−26·0	

Nauru and Eniwetok have been chosen as the illustrative stations because they are further apart than any other two within the barotropic current. The same agreement is found between Nauru and other stations lying north of the meteorological equator and south of 15°N. Wake, however, differs considerably from other Marshall Islands stations and shows moreover systematic variations associated with the passage of a trough in the westerlies aloft. It belongs to what might be called the baroclinic part of the basic tropical current. Bikati, which lies practically on the meteorological equator during this time, agrees with other stations up to 500 mb, but above that level the isobaric surfaces become progressively higher than over any other station. This is associated with the outflow above the meteorological equator at high levels. Table III compares the heights of the standard surfaces at Bikati and Kwajalein on the 29th.

TABLE III. HEIGHT OF THE STANDARD ISOBARIC SURFACES AT BIKATI AND KWAJALEIN, 15 LST, 29 APRIL 1951. HEIGHT IN FT.

Surface (mb)	1,000	850	700	500	400	300	200	150	100
Bikati (ft)	270	4,920	10,310	19,260	24,940	31,900	41,020	47,020	55,050
Kwajalein (ft)	330	4,940	10,300	19,140	24,760	31,650	40,620	46,460	54,090

The previous discussion has entered into perhaps tedious detail with respect to a typical situation in the central oceanic tropics. It is necessary to do this, first, to show the kind of data that the tropics provide under optimum conditions; secondly, the methods of analysis and leading concepts of the school, elswhere termed the perturbation school of tropical analysts, have been amply illustrated; thirdly, only a discussion of this kind can establish the relation of the newer concepts to those represented in most text-books, notions that have been grouped as belonging to the climatological and the air-mass schools.Having done this, we may proceed to an account of the main body of the synoptic theory, without detailed analytic justification. The reader may be assured, however, that unless the contrary is stated, the evidence for apparently dogmatic assertions is of the same kind as that already advanced. Analytic details can be found in the literature, some of it not widely accessible; more particularly, the writings of Riehl and his co-workers, and of members of the Tropical Project, University of California, which are listed in the table of references, should be consulted for synoptic justification.

(b) *The transformation of equatorial waves*

Waves of the type described in sub-section (a) are characteristic of the central Pacific, between the longitudes 160°E. and 150°W. With the network of observing stations usually maintained in the region, most of them escape detection. The amplitude of the disturbed streamlines is so small and the accompanying pressure wave so slight that only networks as dense as those operating during weapon tests can provide observations good enough to pick them up. For tropical forecasting, observations should be particularly dense in the latitude belt extending five to seven degrees on either side of the equator. As a convenient term of reference, such waves may be called stable equatorial waves. The question now arises : what happens to stable equatorial waves after they have passed westward out of the region in which they show the stable characteristics described above ? Fortunately the question can be answered from a study of the Marshall Islands data. For, although the area is affected by stable waves during a greater part of the year, from July to September (and sporadically, at other times) the region in which only stable waves are found moves upstream. At these times the later stages in the development of equatorial waves can be studied within the Marshall Islands network.

For the time being attention will be confined to events occurring in the lower part of the atmosphere, for example at 1,500 ft. Here one of the earliest signs of instability is an increase in amplitude of the streamlines, the isobars and the isovels of the wave. On the left hand column of Fig. 22 is a series of idealized pictures showing the progressive effect of the increase in amplitude of the streamlines of a wave moving in a basic current without an intertropical convergence line. Diagram A shows the unstable wave ; in B the first singularity, known as a cusp point, has appeared, marking

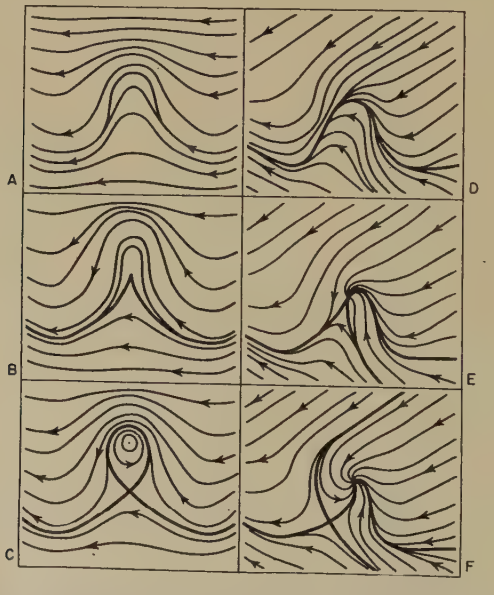


Figure 22. Transformation of the streamline pattern during the formation of a vortex from an equatorial wave. A, B, C in a basic current without horizontal velocity divergence ; D, E, F in a basic current of type shown in Fig. 11.

the transition of the wave into a vortex ; the vortex is shown on Diagram C. Conventionally, the vortex consists of two singular points and their surroundings ; the northern point is an isolated cyclonic singularity, the southern the hyperbolic or neutral point. The closed cyclonic circulation is surrounded by a nodal loop in a special streamline toward which all others either converge or away from which they diverge. Such a line, not necessarily closed, is known as an asymptote. In this case the line is, along its course, alternately an asymptote of convergence and of divergence. It is understood that during the development illustrated the whole system is moving toward the west, usually at a steady pace between 10 and 15 kt.

The sequence displayed on Diagrams A, B and C, Fig. 22 is rarely found on any series of equatorial maps. It is given, as were Figs. 7, 8, and 9, to illustrate kinematic events without the complication due to the general horizontal velocity convergence that accompanies the ' equatorial front.' The effect of increasing amplitude is, as it were, studied in isolation. However, when the unstable wave is moving in a current similar to that shown in Fig. 11, as it usually is, the increase in amplitude is most

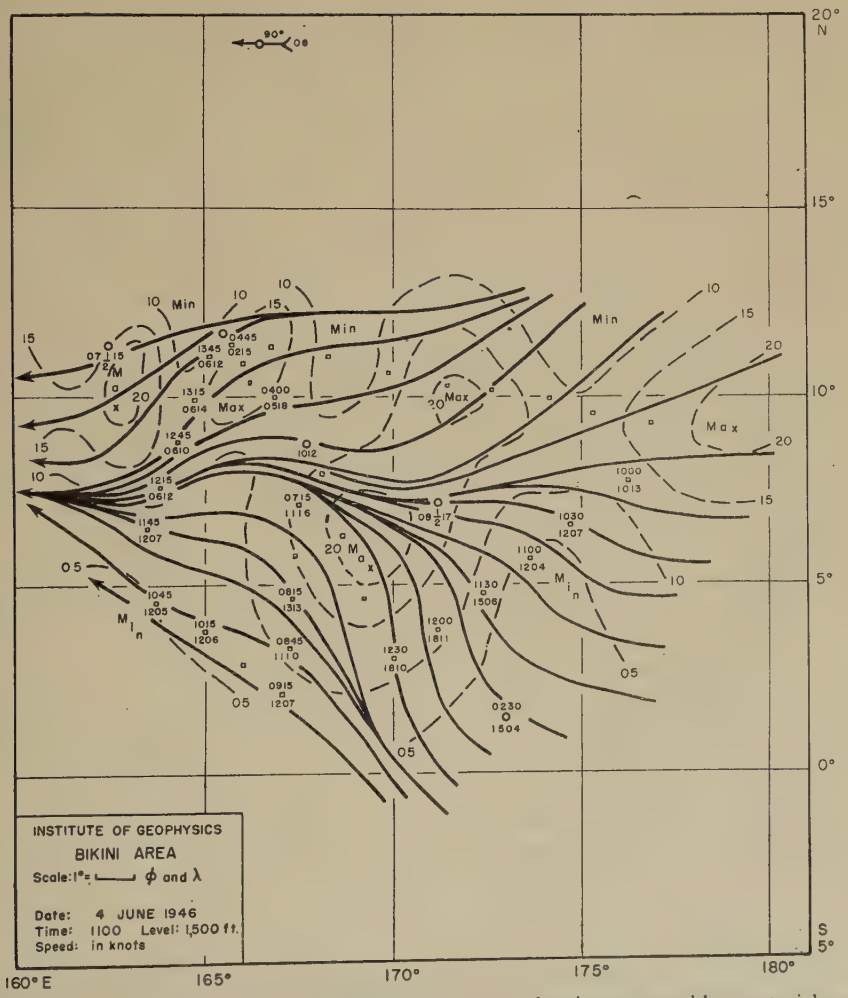


Figure 23. The field of motion at 1,500 ft, 4 June 1946, showing an unstable equatorial wave.

frequently accompanied by a local increase in convergence in the basic current. An asymptote of convergence will appear in the wave, even before the formation of singularities ; it is then often identified on conventional surface maps as the equatorial front. A line of heavy cloud and precipitation, which is an almost invariable accompaniment of this line, will confirm this conclusion in the mind of the air-mass analyst. It should be emphasized, however, that the line is not a discontinuity in the air motion. The convergence of the streamlines in the neighbourhood of the line is so great, that on the scale usual on synoptic maps, the streamlines run together. In fact, although the streamlines are very close together, they will join or cross one another only at singular points, where the wind speed is zero.

The streamline pictures shown on the right-hand column of Fig. 22, especially D and F, occur frequently on definitive analyses of the western North Pacific air motion during the wet season. The pattern on E, since it is only an evanescent

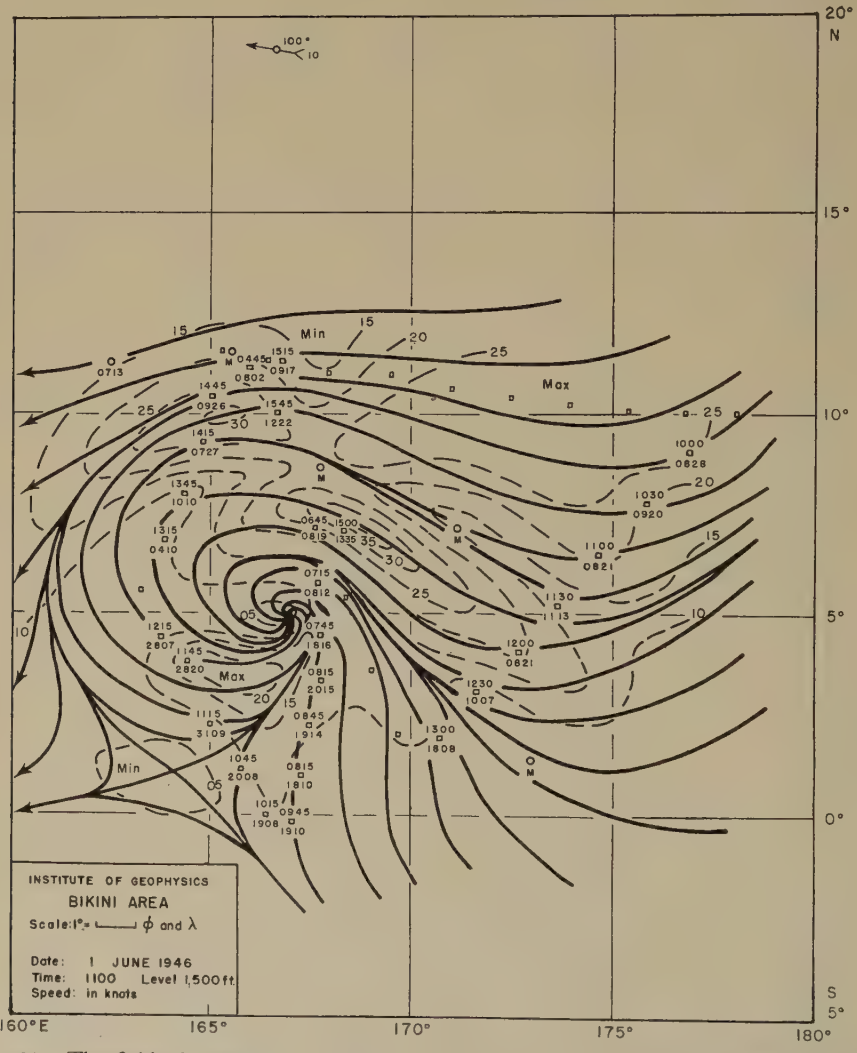


Figure 24. The field of motion at 1,500 ft, 1 June 1946, showing a typical sub-equatorial vortex.

transition stage between D and F, occurs less frequently. Examples are given in Figs. 23 and 24 ; these are taken from the definitive maps for Operation ' Crossroads ' (June and July 1946). On these maps the variation in the isovel patterns associated with these streamline transformations may be seen. As great variation in the speed patterns occurs from case to case, idealized isovels have not been drawn on Fig. 22. In general, an isovel minimum appears at the crest of the unstable wave, even before the development of any singularity ; when the singularities appear, of course, they are enclosed within the 5 kt isovel. Sometimes the area of light winds about the points may be extensive, especially toward the south. In the western Pacific the ' doldrums ' will frequently consist of a train of vortices like that shown on F, Fig. 22, all the singularities of the train lying within the 10 kt isovel. It may be recalled that the early navigators described the doldrums as a zone of light and *variable* winds. But north of the crest of the vortex, values of the wind speed which are higher than any accompanying the stable wave from which the vortex was formed, are usually found. The vortex shown in Fig. 24, for example, is accompanied by a small region where winds are above 30 kt.

During the development just described, the isobars also increase in amplitude, though to a lesser degree than the streamlines. A closed centre can sometimes be detected at stage E, but it requires dense and accurate pressure observations and the drawing of isopleths at one-millibar intervals. The same requirement is also usual at stage F ; the winds are still fairly light south of the crest and pressure gradients are small in that region ; it is a matter of luck if a closed centre should appear on the conventional surface map analysis. For this reason, disturbances at stage F are usually identified on routine maps in Pacific weather stations as ' easterly waves ' of the type described by Dunn (1940) and Riehl (1943) in the Caribbean. The result has been unfortunate : the weather distribution about and in a disturbance such as F will be quite different from that to be expected with a Caribbean easterly wave ; in particular, heavy cloud and precipitation will occur ahead of the positive axis. Forecasts based upon the misapprehension are almost bound to fail.

The transformation of a wave into a vortex takes place first in the lowest layers of the equatorial atmosphere. Aloft, the disturbance still has the form of a stable wave. With varying speed, depending on the rate at which the vortex is intensifying, the transition through unstable wave to vortex extends upward. The fate of the upper wave at 25,000 ft and above, which was described as being in opposite phase to the lower wave, is at present uncertain and should be a topic for future research. There are great difficulties in the way of investigation, however, since the methods of evaluation of the basic current, described in Sub-section (a) are invalid. The perturbation velocities are no longer small in comparison with the basic current so that the mean motion taken over the period of transformation cannot always be interpreted as the basic motion. However this may be, it appears that a vortex which is developing into a typhoon reaches the upper layers very quickly and the circulation can be detected at 25,000 ft even before the surface winds have reached hurricane intensity.

So far we have discussed vortex formation in one hemisphere, which for convenience has been taken as the northern. Events during vortex formation in the southern hemisphere can easily be inferred from the diagrams. Since equatorial waves are disturbances which perturb the basic motion of both hemispheres, the possibility exists that vortex formation may occur at the same time in both. It has long been known (Reid 1849) that tropical cyclones centred within a few degrees of one another can be present

in both hemispheres simultaneously. Certain months in meteorological history are remarkable for this development, e.g., December 1940 in the Pacific. Recent investigations have shown that double-vortex formation is comparatively common in the Pacific, especially in the months of transition between the wet and dry seasons.

At the present stage of development of theoretical meteorology a complete account of the dynamics of the formation of a vortex from an equatorial wave cannot, of course, be given. However, the new observations enable us to determine the orders of magnitude of the various terms in the equations of motion and of continuity and thus to estimate qualitatively the relative importance in vortex formation of the physical processes which those terms represent. This is reached through consideration of air-parcel dynamics in the region between 10°N. and 10°S. in a field of motion already disturbed by an equatorial wave.

For such a consideration the *vorticity equation* can be written in the usual meteorological Cartesian variables x, y, z, the axes of reference being directed toward the east, north and the zenith respectively. If the operator d/dt represent differentiation with respect to time, following an air parcel, and $u = \frac{dx}{dt}$, $v = \frac{dy}{dt}$, $w = \frac{dz}{dt}$, $\zeta = \frac{\partial v}{\partial x} - \frac{\partial u}{\partial y}$ while $f = 2\,\omega \sin\phi$, $\beta = df/dy$, p = pressure, α = specific volume, the rate of change per unit time of the vertical component of vorticity of an air parcel is in frictionless motion

$$\frac{d\zeta}{dt} = -\beta v + \left(\frac{\partial \alpha}{\partial y}\frac{\partial p}{\partial x} - \frac{\partial \alpha}{\partial x}\frac{\partial p}{\partial y}\right) - (f+\zeta)\left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y}\right) + \frac{\partial w}{\partial y}\left(2\,\omega\cos\phi + \frac{\partial u}{\partial z}\right) - \frac{\partial v}{\partial z}\frac{\partial w}{\partial x} \quad . \quad . \quad . \quad (1)$$

Landers (1950) and other workers on the Tropical Project have shown that the orders of magnitude of the terms in this equation are usually, in the region between 20°N. and 20°S., such that the equation reduces to

$$\frac{d\zeta}{dt} = -\beta v - (f+\zeta)\left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y}\right) \quad . \quad . \quad . \quad (2)$$

In exceptional cases the last two terms on the right-hand side of Eq. (1) must be taken into account. We shall restrict our attention to the lower layers of the atmosphere where, as our example shows, $\partial u/\partial z$ and $\partial v/\partial z$ are small quantities.

For the time being the term βv will be omitted, so that :

$$\frac{d\zeta}{dt} = -(f+\zeta)\left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y}\right) \quad . \quad . \quad . \quad . \quad (3)$$

The *direction* of change of ζ therefore depends on the sign of the horizontal velocity divergence, of the initial value of ζ in various parts of the wave system and on the sign and magnitude of f. In the equatorial wave, values of $\zeta = 15{\cdot}0 \times 10^{-6}$ are not uncommon, and values as high as $40{\cdot}0 \times 10^{-6}$ may be found ; further, if the equatorial trough lies close to the equator, the greatest initial values of ζ are found in the region where f is small. A simple computation will show that, under these circumstances, f can normally be neglected in comparison with ζ in the region between 3°N. and 3°S. and, with high values of ζ, near the ' crests ' of the wave, between 7°N. and 7°S.

Near the equator, in fact, we have

$$\frac{d\zeta}{dt} = -\zeta\left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y}\right) \quad . \quad . \quad . \quad . \quad (4)$$

Table IV shows the dependence of the sign of $d\zeta/dt$ upon the signs of the initial vorticity in the wave and the divergence.

TABLE IV.

$\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y}$	ζ	$\frac{d\zeta}{dt}$	Effect
+	+	−	Damping
+	−	+	Damping
−	+	+	Amplifying
−	−	−	Amplifying

When ζ and $d\zeta/dt$ have opposite signs we may legitimately speak of a damping effect, since the vorticity of the parcel moving in a part of the field of motion where these conditions obtain will diminish or increase toward zero with time. The opposite effect, similarly, is that of a continual departure from zero, in the sense of the initial vorticity, in the appropriate part of the field. This we shall call the amplifying effect.

The great importance of the horizontal velocity convergence in the basic current is now evident. Convergence alone has an amplifying effect, divergence will tend to damp the parcel vorticity. In a field such as that shown in Fig. 12 instability may well be brought about by an increase in the convergence of the basic current. The result will be the formation of a counter-clockwise eddy at the northern bend in the streamlines of the wave and a clockwise eddy at the southern bend. If the intertropical convergence line or 'equatorial front' corresponds in position with the geographical equator, cyclonic vortices, one in each hemisphere, will form. If the meteorological equator lies in one hemisphere, say the northern, but still within the zone 0°-7°N. the result will be the formation of northern cyclonic and southern anticyclonic vortices. The formation of paired vortices of the latter type has been well described by Riehl (1948); it is taken up again in the next section.

The term βv which is usually one order of magnitude lower than $\zeta\left(\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y}\right)$ will, at least in early stages and the lower layers, have the same effect as the convergence. For in the lower basic current, v is directed toward the meteorological equator. βv therefore, is negative north of the equatorial front, positive south of it; the term reinforces the effect of convergence on both the clockwise and the counter-clockwise eddies. Aloft, however, where βv is of the same sign throughout the basic current (positive at the western ends of the semi-permanent anticyclones of the northern hemisphere, negative at the eastern) the effect of this transport term will be alternately amplifying and damping on the northern counter-clockwise and southern clockwise eddies and the effect will vary according to the positions of the semi-permanent anticyclonic cells of the two hemispheres. The positions and intensities of the upper 'break-through' currents, therefore, may determine selectively whether tropical cyclones are going to develop in one or the other hemisphere.

The effect of the Coriolis parameter must now be considered, the discussion being restricted to the case where the meteorological equator lies north of the geographical equator. Since f is positive it will reinforce the amplification due to convergence in the case where ζ is positive, oppose it when ζ is negative. We should therefore expect the circulation about the cyclonic vortex to increase more rapidly than that about the anticyclonic ; the ultimate effect, if the system of the paired vortices has a northerly component of movement, will be to augment the cyclone and diminish the anticyclone. In higher latitudes frictional cross-isobaric flow will tend to replace convergence in the lower layers of the anticyclonic vortex by divergence, with consequent damping, while its effect on the cyclone will be to increase the circulation. These anticipations are all confirmed by synoptic experience. The great majority of the cyclones of the tropical North Pacific form in latitudes south of 6°N. but intensify rapidly only in the zone 6°N. to 15°N. The anticyclonic eddies are always weak ; while they may persist for several days they do not intensify and appear not to have the long-life characteristic of tropical storms or typhoons.

If the above reasoning is correct, the atmospheric regions adjacent to the equator are analogous to the bath-tub in the old problem concerning the direction of circulation of a vortex formed during outflow from a vessel. In the atmospheric case, there is a linear sink in the basic current at the meteorological equator. The initial distribution of vorticity is given as a result of the stable equatorial wave. The conditions for the formation of the wave are probably other than those contemplated here ; similarly the transformation of a cyclonic vortex into an intense typhoon involves other considerations connected with energy transformations in the atmosphere (Riehl 1951). But the instability of the equatorial wave and its transformation into paired vortices may on the present view be regarded as due to a process by which vorticity of either sign is *concentrated* locally in different parts of the wave.

(*c*) *Tropical cyclones*

Only a small proportion of the cyclonic vortices that are being formed throughout the year from equatorial waves attain such intensities as cause them to be classed as tropical storms or as hurricanes. The problem of hurricane or typhoon formation is not therefore identical with that of vortex origin. It is the problem of the almost explosive deepening of an already existent vortex. Although we cannot at present unequivocally describe the necessary and sufficient conditions for the process to occur nor assign dynamic reasons for it, the work of the perturbation school has clarified the nature of the problem enormously. All tropical storms whose origin can be traced begin as the result of instability in a wave in the easterlies ; either an equatorial wave or the easterly wave of the Caribbean, which, as will be shown, is a particular local transformation of the equatorial wave. The kinematic features of vortex intensification are now well known. The thermodynamic transformations will be found in Riehl's paper (1951).

Intensification of the cyclonic vortex is accompanied by a rapid separation of the cyclonic and the neutral singular points in the lower wind fields. As reference to Fig. 22 will show, this is reflected in a rapid increase in the *area of westerly winds* on the equatorward side of the developing cyclone. If double vortices are developing, the region of west winds may become very extensive, overlapping the equator and giving rise to strong west winds on and across that latitude. At Nauru, for example, west winds of 25 to 30 kt are by no means uncommon during the so-called ' westerly season,' i.e.,

the season when double cyclonic-vortex formation occurs frequently in the longitudes of the central Pacific. Double vortices may occur at any time of the year but are most likely during April, May, November and December. The second accompaniment of intensification, of course, is a general increase in wind speed about the cyclonic singularity ; not only does the maximum wind speed in the region increase, but also the broad area of light winds which previously extended equatorward from the vortex crest contracts rapidly to a small region surrounding the singularity – the incipient ' eye ' of the storm. The third development is the rolling up of the chief asymptote of convergence around the ' eye ' to form a more or less circular asymptote of convergence which cuts off the ' eye ' from the stronger winds outside. At the same time several subsidiary new asymptotes appear in the outer circulation and spiral in toward the central circular asymptote. Since the asymptotes are lines along which the horizontal velocity convergence reaches maximum values, they are usually the sites of long lines of cumulonimbus, with heavy rain. They therefore show up on radar screens of the typhoon and hurricane-reconnaissance aircraft. The study of the changes of intensity, form and movement of these lines is in its infancy but it promises to be a fascinating one. Already it is clear that the circular asymptote around the eye may not be complete until quite late in typhoon development ; sometimes it appears to be incomplete toward the southwest (in the northern hemisphere) throughout the life history of the storm. This means that aircraft can fly from the southwest into the eye of such storms without encountering the heavy weather of the inner wall of the typhoon cloud.

In the western North Pacific typhoons may occur in any month of the year ; by far the greatest number, however, are found in the months August, September and October. During the typhoon season a long train of vortices passes continually from the longitudes of the Marshall Islands on to the Asiatic coast. Some of the cyclones reach typhoon intensity and either pass into China and Indo-China or recurve toward the higher latitudes of Japan, Korea and the Aleutians. At an early stage, before the typhoons have, as it were, detached themselves from the vortex series, the asymptotes of the train of cyclones form a continuous line of convergence running ESE. to WNW. in the western Pacific. This line can be readily identified on routine weather maps, again as the equatorial or inter-tropical front. However, over the open sea it is impossible to detect temperature contrasts at the line and there is no reason to suppose that it differs in any way from the asymptotes of convergence in lower tropical latitudes. We see therefore that the term equatorial front (or intertropical front) has been applied to the following entities :

(i) The asymptote of convergence, coinciding with the meteorological equator and its pressure minimum, that is found on climatological maps in the central Pacific but not on synoptic maps. It is a feature of the basic current.

(ii) The asymptote of convergence that appears in an equatorial wave during unstable growth in amplitude.

(iii) The ' separatrix ' or line of convergence that always appears in the field as soon as a vortex is formed. If there is a train of vortices the line may be continuous from one vortex to another.

(iv) In regions of poor observation, the (inferred) line of separation of the westerlies on the equatorward side of a train of vortices and the easterlies north of their cyclonic singularities. Detailed isogon analysis wherever it can be performed reveals the vortex train, lying along the supposed line of separation.

(*d*) *The evolution of kona storms*

There is now a small but important literature on *kona* storms. These synoptic entities appear on surface-pressure maps as rather weak depressions, usually stationary for several days, in the higher tropical latitudes to the west of the Hawaiian Islands. In spite of its innocuous appearance on a pressure map, a kona storm is frequently accompanied by surprisingly heavy rains in the Hawaiian group and sometimes severe squalls cause damage. It used to be customary to attempt a frontal analysis of the storm ; unfortunately, it is usually difficult to draw a front in an appropriate position before it develops. Further, if the attempt at a frontal interpretation is successful at an early stage, the analyst is embarassed by the fact that the cyclone goes on developing *in situ* for days after the occlusion process has been analytically completed on the map. The frontal interpretation is now falling into disuse as a result both of a better understanding of the structure and development of the circulation in the upper layers and of Palmén's investigation of upper-level cold lows (Palmén 1949). A large proportion of kona lows can be traced to upper-level cyclones which develop aloft over several days without definite trace at the surface (Simpson 1951). It is further known that kona rains occur in Hawaii without the appearance of a depression on the surface map ; in such cases it is usually easy to demonstrate the presence of a cold low of the type described by Palmén at and above 20,000 ft. It is now almost routine in Hawaii to analyse kona storms according to the model advanced by Palmén, i.e., to attribute an extra-tropical origin to the storm either by movement from the North American coast of a previously existing cold low or, in the manner of Palmén, by the ' cutting off ' of the southern portion of a pool of cold air advancing from high latitudes.

Recently it has become clear that all upper-level cold lows in higher tropical latitudes are not formed in these ways. There is more and more evidence that cold lows can form, probably from troughs in the westerlies above 40,000 ft, in very low latitudes and remain stationary south of 15°N. for many days. The circulation is initiated at high levels under circumstances that preclude the possibility of their connection with a cold outbreak in which differential subsidence has taken place – the explanation of Palmén. A proportion of kona lows, it is evident, are simply high-level cyclones of this type which have moved from the Marshall Islands in the levels above 20,000 ft, have become stationary in the Hawaiian region, and by deepening aloft and extending downward, have finally given rise to the weak depression at the surface that is characteristic of this class of storm.

It was proposed in a recent paper (Palmer 1951a) to call such lows upper-level cyclones of tropical origin in contrast to those described by Palmén which are qualified as ' of polar origin.' The greatest interest now attaches to the lows of tropical origin during the time they are forming and moving at 12°N. to 15°N. In the Marshall Islands they are characteristic of the ' dry season ' or ' winter ' situation. In the lower layers this situation (which may occur in any month, but is most frequent, of course, in the winter) is characterized by a very low base of the westerlies, by fairly strong northeast trades and by the presence on the lowest maps of a stationary or very slowly moving wave-like disturbance in the streamline pattern. The lower disturbance has been formerly described as the *polar trough* ; it is often possible to trace its connection on the surface map with a frontal trough in the westerlies at higher latitudes and it is still common to see the front at the surface continued into it through the anticyclonic belt of middle latitudes. But, as just pointed out, the polar trough is frequently stationary ; at other times it is seen to move eastward against the trades – it is quite rare to

find a westward movement. The attempt to apply frontal concepts to the analysis of the polar trough leads to intolerable difficulties in forecasting the changes in movement and intensity that such troughs display. It is always possible after the event to postulate acceleration fields which might conceivably have moved the 'front' against the lower easterlies but quite impossible in practice to anticipate the movement through frontal analysis.

The picture becomes clearer when we ascend to higher levels in and to the west of the region in which a polar trough appears at the surface. Almost all marked troughs will be found to be lower level reflections of a closed cyclonic circulation at or above 20,000 ft. The more intense the upper-level circulation and the lower the level at which a cyclonic singularity may be found, the more marked will be the surface reflection. Ordinary 'troughs in the westerlies' which are found between the anticyclones of upper-level tropical maps will have only slight reflections in the lower-level fields of motion, *unless* they are connected with an upper-level cyclone to the south. For example, reference to Figs. 5 and 6 and comparison with Figs. 19 and 20 will show the usual situation that obtains below a trough in the westerlies that lacks this cyclone. The lower winds at Wake (19°N.) were affected by the low-pressure trough associated with the passage of a frontal system in high latitudes and the corresponding col between two anticyclones at the 25,000 ft level. South of Wake, however, it is difficult if not impossible to see any effect of the system on the field of motion below 20,000 ft. A 'deep easterly' situation prevails in the sub-equatorial zone.

The situation is quite different if an upper-level cyclone is present or is forming in the sub-equatorial zone. Because the cyclone is stationary or very slowly moving, it can be regarded as a feature of the basic current. Equatorial waves will still pass from east to west in the latitude belt between the 10th parallels and the system of vertical motions associated with them must still be taken into account when forecasting the weather. But the field of motion and the cloud and precipitation will be dominated by the presence and slow development of the upper-level cyclone. Sooner or later the upper system will move out ; in the winter this movement will be usually toward the east and north ; occasionally during the summer an eastward and northward movement occurs.

3. Application of the hypotheses

(*a*) *A note on technique*

The history of research in tropical meteorology during the past twenty years could be summed up as an account of struggle ; the struggle to free the science from the domination of concepts derived from the study of weather in high latitudes. The domination itself is a recent thing, a consequent of the overwhelming success of the air-mass theory in the area of its origin. Tropical meteorology used to provide leading ideas. The concept of the general circulation arose from early study of the trades and monsoons ; the notion of the cyclone as a synoptic entity, as well as the name, enters meteorology from the study of low-latitude weather. The great difficulty of applying air-mass techniques to hurricanes and the success of the easterly-wave analysis in the Caribbean after the publication of Dunn's paper (1940) finally freed the science from extra-tropical bondage and led to the development of that complex of hypotheses that I have elsewhere attributed to the perturbation school.

If we examine the *techniques* of tropical meteorologists over the past twenty years, however, we can see that Dunn's paper made explicit ideas that were already latent

in the practice of the few professional forecasters who had to deal with tropical weather. Whatever their published views might have been and however closely they scrutinized the observations for temperature contrasts, their main practical preoccupation was with the wind field. With the weak pressure gradients usual in low latitudes, with the almost complete lack of systematic temperature gradients of the kind demanded by the air-mass theory and with the failure (as was ultimately realized) of even the gradient-wind relationship close to the equator, the isobars were in fact crude streamlines to which were assigned pressure values that roughly agreed with the barometric readings in the vicinity. The primacy of the wind field was already evident in the work of Grimes (1944) and Deppermann (1936). If all references to the mass field had been omitted from their works, their pioneer contributions to tropical synoptic studies would remain unchanged. It is significant that Grimes's attack on the dynamics of equatorial perturbations invokes principles dependent upon the *autobarotropic* transport of vorticity – at a time when this concept had hardly begun to penetrate dynamical theory of high latitudes.

The transition from 1940 (Dunn 1940) to 1943 (Riehl 1943) marks the explicit acceptance of the analysis of the field of motion as definitive. The war-time observations were still not sufficiently reliable, dense or independent of weather to permit more than a qualitative streamline analysis, but the importance of horizontal velocity divergence, as given by the wind field without dynamic assumptions, had been realized. Streamlines were begun and ended in the field to indicate in a crude but approximately correct manner the areas of pronounced velocity divergence.

With the development of the rawinsonde, particularly the latest self-tracking and semi-automatic types, and with the introduction in a limited area in the central Pacific of a reasonably dense network of aircraft and ground observations and with the establishment of hurricane and typhoon reconnaissance by specially trained weather crews, the possibility of applying the beautiful techniques of isogon analysis to tropical wind fields has been realized. These techniques were developed long ago (Sandström 1909, V. Bjerknes *et al.* 1911); they have been dormant, as it were, awaiting the revolution in weather observation that is now following the invention and improvement of radar.

The result of these technical changes has been to bring descriptive tropical meteorology again into the foreground. There is so much to describe anew and to correlate with earlier findings that there has been little time to follow up the theoretical implications of the new descriptions. The route of progress in the future is plain: it lies in the extension of an upper-air network of the type used during the weapon testing in the Marshalls to wider and wider areas in low latitudes and in theoretical studies leading to the explanation of tropical basic currents and their perturbations.

An understanding of these technical changes over the past twenty years is necessary before the attempt is made to correlate old and new research and to incorporate them in a unified synoptic theory of the tropical circulation. To a newcomer to the field, an analysis of a situation in the western North Pacific by Deppermann appears to be utterly different from that produced for a similar situation by a member of the perturbation school, say Riehl. The streamlines that result from the application of the isogon method produce another shock of unfamiliarity; again there appears to be a qualitative difference in technique. The reader can be assured that this is not so. It is a fairly simple matter to translate one into the other. The advantage in using the isogon method lies in the fact that when the observations are dense enough and accurate

enough, the changes on wind-time sections can be accurately transferred to the analysis and computations leading to a quantitative knowledge of the fields of vorticity, divergence and vertical motion can be performed on the maps. The method *cannot* give better results when the observations are widely spaced and inaccurate, as they generally are in the tropics, any more than the air-mass method of analysis could be used in higher latitudes if the observations were on the average 1,000 mi apart. To my knowledge, the Sandström analysis has been used as a routine method only in some weather stations of the Marshall Islands and that only during the periods of dense observation.

On the other hand, the method is very useful in research, when there is time to use it with great care. The results of its application under these circumstances are not irrelevant to routine weather analysis in the tropics. For, although it would not be possible to discover new synoptic facts by its application to the poor networks at present maintained, the models that come from its use in research can be applied to current maps. To this application we now proceed. It must be remembered, however, that in this context the models represent a set of working hypotheses. We cannot be sure that they are applicable outside the western Pacific ; the most that can be done is to show that, leaving the question of full confirmation open until the day when reasonable upper-air observing networks are constituted, they consistently explain known synoptic facts in many other areas.

(*b*) *The working hypotheses in summary*

There is a large measure of agreement among workers of the perturbation school ; the only disagreements, minor in extent, concern matters lying near the frontiers of research. Without committing other members of the group, I am here attempting to summarize the working hypotheses upon which, I think, they would agree. The detailed investigations can be found in the literature (Dunn 1940, Riehl 1943, Simpson 1945, Riehl and Schacht 1946, Sherman 1950a, b and 1951, Dean 1951, Landers 1950, Dean, Stopinski and Miller 1951, Palmer and Dean 1950, Palmer 1949). For brevity of expression, the hypotheses are given with reference to the central and western Pacific ; it is true, of course, that some of them were first derived from the study of data from the North Atlantic, the South Pacific and the Caribbean Sea and Gulf of Mexico. With this understanding, the hypotheses are :

(i) The basic current of the Pacific consists of two parts : a lower sub-equatorial current, predominantly easterly in direction and to a high degree of approximation *barotropic* ; the upper current is increasingly *baroclinic* as one proceeds upward and poleward away from the equator. There is no sharp boundary between the two parts of the basic current ; roughly the transition lies in the lowest layers about the 15th parallels of latitude ; it then slopes upward to a dome between 20,000 ft and 30,000 ft over the meteorological equator. Over the meteorological equator, the dome may, during periods of ' deep easterlies,' be as high as 40,000 ft.

(ii) The axis of the barotropic current, not necessarily symmetrically placed, is what we have called the meteorological equator, and this must in all months be distinguished from the geographical equator. On this axis the wind speed and the pressure reach their minima. The easterly wind-speed *maxima* lie on the outer boundaries of the barotropic current, usually below 10,000 ft. The basic current therefore has cyclonic shear throughout its breadth at times when the meteorological and the geographical equators coincide. In the barotropic current the meteorological equator is

also marked by the presence of an asymptote of convergence, which has been called the equatorial front, the intertropical front and the intertropical convergence. Here the horizontal velocity convergence which is a characteristic of the whole lower barotropic current, reaches its maximum. However, the asymptote may not be present as such on any individual streamline map ; it is a feature of the basic current. Its presence in the underlying current may almost always be detected, frequently by the methods outlined in Section 2.

(iii) The baroclinic part of the basic current is distinguished by decreasing easterlies, ultimately turning to westerlies at altitude. Close to the meteorological equator and at high level above 30,000 ft it is also marked by the *return current* which appears to be present on all longitudes so far investigated. The term anti-trade is not used here, since it has been applied in many different contexts. Heat transported upward and outward in the return currents may result in the inflation of the isobaric surfaces above 30,000 ft over the meteorological equator and in the general baroclinity of this part of the tropical current.

The simplicity of the direct cell is obscured by long waves in the baroclinic current. These long waves are the tropical portions of the great anticyclones, both stationary and moving, which are centred at the surface in the subtropics. The cells alternate in the two hemispheres, the centres of those in the southern hemisphere being on approximately the same longitudes as the cols between those of the northern. This is more particularly the case with the stationary perturbations. The result is that there is superimposed upon the direct cell a flow from one hemisphere to the other, alternately in one direction then the other – this is the ' break-through ' meridional component of the motion characteristic of the layers between 20,000 and 35,000 ft.

(iv) The axial regions of the tropical current, both barotropic and baroclinic, are the seat of westward-moving *equatorial waves*. The instability of these waves results in the formation at low levels of warm-core vortices which continue on the westward track – sometimes extending upward into the upper baroclinic current and on to the tropopause. The more intense deep vortices are known as typhoons. The equatorial waves reach their greatest amplitude at the meteorological equator and are rapidly damped on either side. The intensity of the direct cell determines the region in which the waves become unstable and the speed and limit of intensification of the vortices. In addition, the position and intensity of the long baroclinic waves aloft exert a selective influence on formed vortices (see Riehl 1948) and probably have much to do with the passage from weak vortex to typhoon. The mechanism by which the long waves exert their influence is probably to be sought in the ' latitude ' effect on the break-through currents (the term βv in Eq. (1)).

(v) The outer and upper baroclinic parts of the current are also subject to short-wave perturbations. Riehl (1948) has made a particular study of these disturbances. The cyclones are of the cold-core type ; they increase in intensity aloft, usually above 30,000 ft. In contrast to the equatorial waves their movement is irregular ; they may remain stationary for long periods or move out of the tropics into higher latitudes. The study of the systems is in its infancy, but it is known that some of the kona storms of the Hawaiian region are derived from them (Palmer 1951a).

(vi) In intensifying, equatorial vortices extend upward from the surface layers ; cold lows in the baroclinic current extend downward toward the surface.

(vii) Rapidly deepening warm-core vortices probably derive their energy from latent heat of vaporization. The energy source of cold-core lows is unknown.

(c) *The Atlantic Ocean*

Since some at least of the hypotheses of the previous sub-section were first derived from a study of the Caribbean, and later applied to the Pacific, it is relatively easy to apply them to the western part of the Atlantic. The first and most important difference between the oceans has long been known ; the meteorological equator in the Pacific has a great seasonal swing in the west from one hemisphere to the other while in the western Atlantic, so far as is known, it is never found south of the geographical equator. In the Pacific tropical cyclones are found in both hemispheres, the maximum number per month occurring in late summer. But in the western Atlantic tropical cyclones occur only north of the equator. Application of the previous theory would indicate a causal relation between these facts. Instability of equatorial waves in the region 0°N. to 10°N. in mid-Atlantic would result in paired vortices, but, of these, for the reasons outlined in Section 2 (b), the northern members would be cyclonic ; these, under appropriate conditions, would intensify, while the southern anticyclonic eddies would be subjected to damping forces which would keep them weak or perhaps would prevent their overt appearance on synoptic maps. It is difficult to check whether the clockwise eddies occur at all in the Atlantic. In southeastern Asia there is one of the heaviest concentrations of human population in the world, and, while the observations in the area still leave much to be desired, there is sufficient information to delineate the major features of the circulations. There are, moreover, thousands of islands scattered in the archipelagos of the central and south Pacific and on many of these the weather is observed and reported. In the sub-equatorial Atlantic, however, there are very few islands and Brazil, the Guianas and Venezuela are thinly populated countries. On the basis of the hypotheses we should predict that when observations become dense in South American countries and when weather ships are at long last stationed in the tropical parts of the Atlantic, the clockwise vortices will be found in the region between 0°N. and 10°N. Observations taken during the war along the eastern tropical coast of South America, when combined with pilot reports from the many transport aircraft that crossed the sub-equatorial Atlantic toward the Middle East, suggested strongly that there were clockwise eddies accompanying the more intense Atlantic storms and hurricanes. The possibility exists that some of the clockwise eddies, more particularly those very close to the equator, may recurve southward over Brazil and after crossing the equator, form part of the ' monsoon low ' that during the summer extends over the interior of the continent as far north as Panama and as far south as Bolivia. Undoubtedly this low-pressure area is partly determined as an orographic feature, both as an effect of the Andes on the high latitude westerlies and partly as a thermal low. But the heavy monsoon rains of the Amazon valley are surely not different in origin from those of southeast Asia. With the latter much evidence goes to show that the monsoon rains are not entirely orographical in origin ; heavy precipitation usually accompanies the arrival of a vortex, not necessarily of great intensity, which, having formed thousands of miles out in the central Pacific and moved westward, at last finds its resting place in the interior of the continent (Palmer 1951b). The Brazilian rains might well be the result of a similar development. The northern parts of equatorial waves can be detected on occasion at the surface of the tropical Atlantic ; attention has chiefly been paid to those that, increasing in amplitude, show up in the Caribbean. However, the observations at the only station giving consistent high-level winds from the Amazon basin – Manaos – suggested strongly the westward passage of weak eddies of both kinds over the continent during the season of rains.

Hurricanes are divided into two classes : those that form out in the Atlantic by a process similar to that already described and those that develop in the Caribbean or the Gulf of Mexico from the easterly waves described by Dunn and Riehl. The latter usually cover a much smaller area than the former with winds of hurricane force, but within that area are more intense ; they also deepen more rapidly than most tropical cyclones, sometimes attaining full intensity within 12 hr of the initial vortex formation. What, then, is the relation of the easterly wave and the Caribbean hurricane formed from it to the equatorial waves previously discussed ? The first and most obvious difference is that the initial vortex formation takes place at a much higher latitude ; the instability of the easterly wave occurs in the latitude belt between the 10th and 30th parallels. We can hardly expect that the instability in these latitudes is to be attributed to the same relative disposition of forces as in the sub-equatorial zone. The winds are approximately gradient winds ; Coriolis force is no longer negligible. The dynamic explanation of the characteristics of the easterly wave is therefore that advanced by Riehl, following J. Bjerknes (Riehl 1943, Bjerknes 1937). The theory was fully treated in an extended paper published later (Bjerknes and Holmboe 1944). Certain modifications have been suggested to me by Professor Bjerknes ; they are outlined in another place (Palmer 1951b, see the footnote on p. 870).

The rapidity with which the vortex deepens suggests that the instability of the easterly wave is a baroclinic phenomenon. This is confirmed by a study of the vertical structure of the easterlies that prevail for extended periods during the hurricane season over the Caribbean and the Gulf of Mexico. In the late summer and autumn the warmest part of the troposphere in the North American sector lies over the Gulf Coast and Florida. Temperatures in the upper troposphere decrease southward over the Caribbean as far as South America. The easterlies are deep, since the isobaric surfaces reach their maximum heights near the 25th or 30th parallel, and winds are quasi-gradient. An equatorial wave that has been moving across the Atlantic, on reaching the area with a reversed temperature gradient (west of 75°W.) finds itself in a region where the normal baroclinic damping associated with the westerlies aloft has been replaced by a deep easterly current, with cyclonic shear up to great heights. The disturbance extends rapidly northward into these deep easterlies. In a barotropic current the axis of the wave will be almost vertical ; the slightest alteration in the north-south gradient of mean temperature, then, may be expected to lead to a slope of the wave axis with height. If the slope is eastward with height, as it is usually observed to be, the positive axis or trough line at the surface will lie under a region where the mass divergence is positive to great heights and this divergence, if great enough to overcome frictional convergence in the lowest layers, will lead to a rapid fall of pressure at the surface. An almost explosive vortex formation and deepening will take place.

On this view easterly waves of the Caribbean type are to be expected in regions and at times when the normal temperature gradient in the baroclinic part of the tropical current is reversed in sign – they are poleward portions of equatorial waves which have extended under these peculiar conditions into high tropical latitudes. It seems that such conditions, while they cannot be excluded over the great oceans, are most likely to occur in summer and fall over seas that lie on the equatorward side of continental masses. The Gulf of Mexico and the Caribbean Sea are ideally placed, but it is to be expected that similar conditions could prevail south of Baja California, between the equator and the northwest coast of Australia from Broome to Onslow, in the South China Sea, in the Bay of Bengal and in the Arabian Sea. These are all known as regions

lying outside the chief tropical-cyclone belts of the great oceans and in relatively high latitudes where small dangerous storms may develop more or less explosively. It will be an interesting topic for future research to determine whether the conditions in the basic current during periods of storm formation are the same in these areas as in the Caribbean.

There remains one question : is there any synoptic entity in the Atlantic corresponding to the kona low of the Pacific ? There are so few upper-air observations in the tropical Atlantic that it is difficult to answer the question with any certitude. However, under favourable circumstances the Antilles observations, especially those from Trinidad, indicate the formation and deepening of an upper-level low of a type quite similar to that already described for the Pacific. It seems that such lows can move northeastward toward the Azores, coming to the surface south of those islands. A long-lived almost stationary cyclone is the result. Figs. 25 and 26 show a situation during which the development of a cold low of tropical origin is suspected. Similar

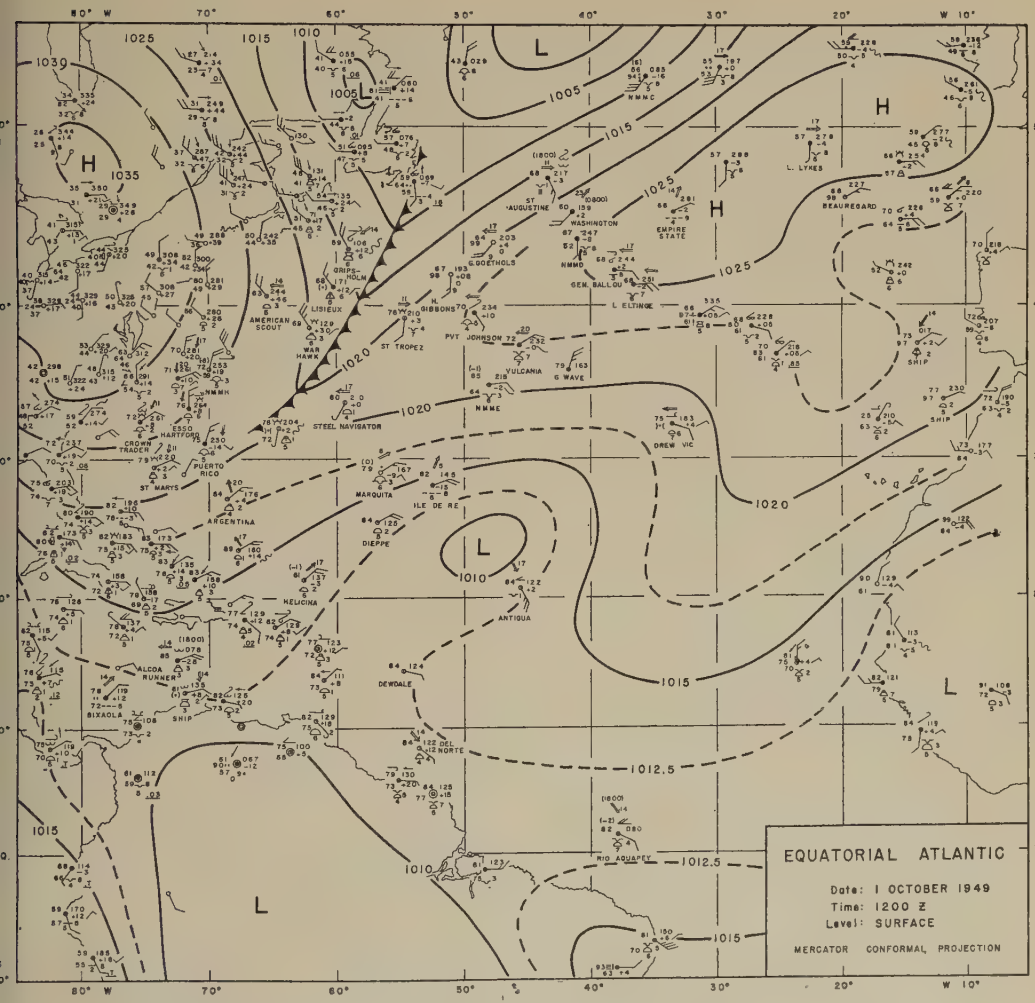


Figure 25. Surface analysis, western North Atlantic 12 GMT, 1 October 1949.

situations may be found in the *Northern Hemisphere Historical Map* series (H.Q., United States Air Weather Service, 1949 et seq).

So often does this explanation of synoptic events, often called ' low index,' seem to be applicable in the Atlantic that it is staggering to find little interest among meteorologists in North America and Europe in tropical Atlantic observations. In former times meteorologists used to reproach aviation authorities because they so slowly grasped the idea that forecasts had to be based upon observations not only along scheduled aircraft tracks but also off the routes. In a more subtle context, meteorologists are afflicted by the same short-sightedness. If one were to judge, for example, by the manner in which funds available for weather ships are allocated, one would have to conclude that atmospheric events in high latitudes are the only ones worth exploration by rawinsonde and that those in the tropics are of importance only to tropical meteorologists. The tropical forecaster knows that he cannot understand the atmosphere in low latitudes without a knowledge of that which lies beyond the 30th parallel ; he suspects that the converse is also true.

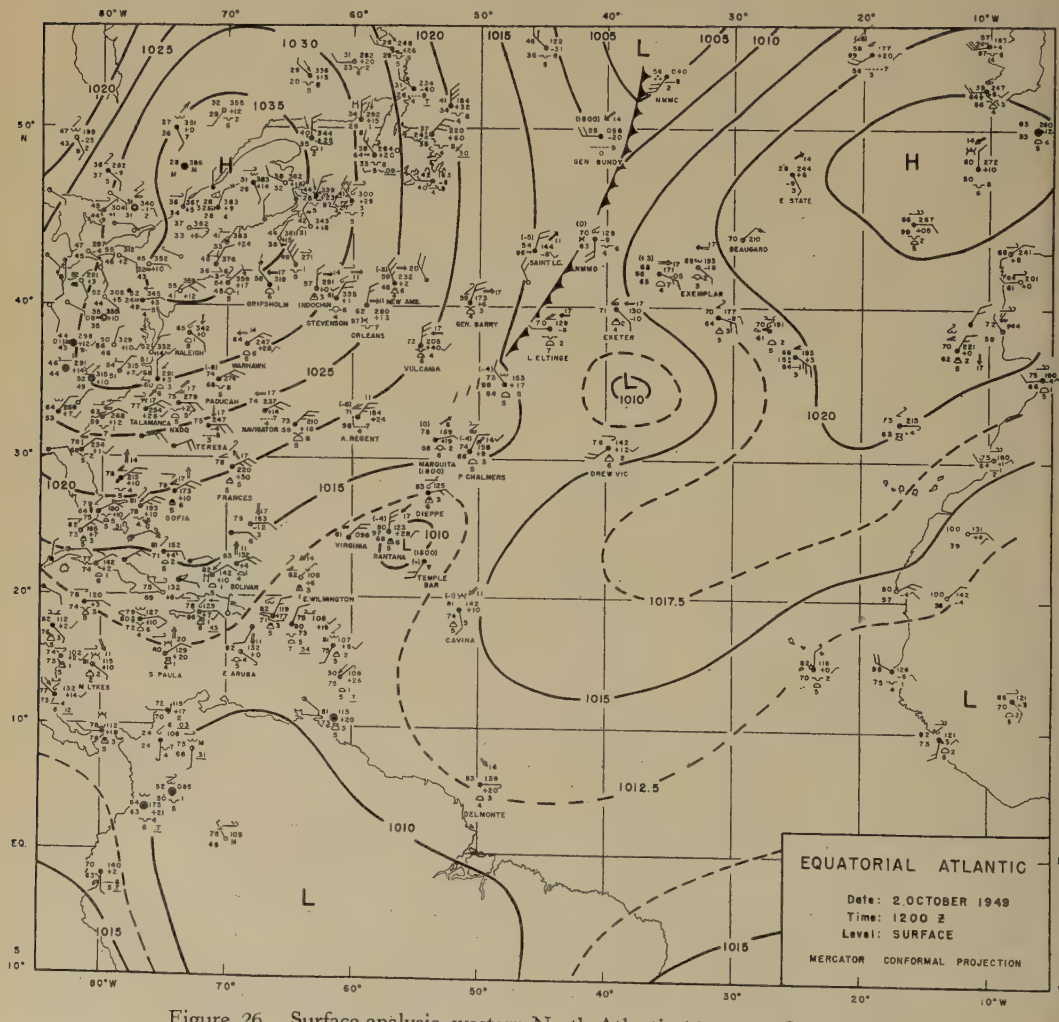


Figure 26. Surface analysis, western North Atlantic 12 GMT, 2 October 1949.

(*d*) *The Continents*

Some apology is now necessary for the emphasis on oceanic research in this review of recent advances in tropical meteorology. An excuse could be based on the fact that much of the material summarized here is derived from contract reports not readily accessible to meteorologists, especially those outside the United States. Elsewhere there has been little investigation of oceanic tropical meteorology since the great days of Piddington and Reid. In the past few years British meteorologists have been chiefly interested in regions under the sway of the monsoons – Africa, India, Burma, Arabia and the Persian Gulf. Here also the war extended the reach of observation and investigation has passed the exploratory stage. Since I have no direct knowledge of these regions I have to depend upon published accounts, combined with some few re-analyses of the data displayed in the literature.

From this literature, the reason for omission of an extended discussion of the monsoons is obvious. While the frontal explanation of tropical perturbations, and the air-mass analysis based upon it, have had to be abandoned in the Pacific and in the western Atlantic, it has, according to many of the authors to be cited in this sub-section, continued to be very successful in countries bordering the Indian Ocean and in western Africa. True, as pointed out previously (Palmer 1951b), there have been modifications of the theory and great complexity has been introduced in places into the original simple account of the equatorial front. But the fundamentals of the air-mass theory as applied to the tropics, and as described by Brooks and Braby (1921), by V. Bjerknes and others (1933) and by Grimes (1938) have remained unchanged. The reader may refer to standard text-books (e.g., Pettersen 1940) for a summary of the tropical air-mass explanation. Grimes (1951) has recently reiterated the theory.

The advances in monsoon meteorology, then, have been in the application of frontal analysis to regions newly opened up for exploration. Literature quoting the war-time observations is rather scanty. Forsdyke (1949) has an excellent summary and a detailed study of East African fronts (1948). The theory has been extended to western Africa (Hamilton and Archbold 1945, Schove 1946), to northern Australia (R.A.A.F. Meteorological Services 1944) and to the higher atmosphere over India (Sawyer 1947). The most comprehensive application, however, has been that of Pendal (Met. Off., London 1943) based not upon new observations but on a series from 1937. From the analyses published in Vol. 3 of Pendal's work it appears that the atmosphere over the tropical Indian Ocean and the monsoon countries is much more complex than elsewhere, consisting in both seasons of many air masses interacting at numerous frontal boundaries. It is suggested, and the maps are analysed on this basis, that the equatorial front is an ' envelope ' of high-latitude fronts that advance from the temperate zones of both hemispheres up to the meteorological equator, where they are wrapped on to the equatorial front. The latter seems to be a permanent discontinuity appearing on every synoptic map as a continuous boundary separating the air masses originating in the South Indian Ocean from those of the north.

No Pacific meteorologists would presume to suggest alternative explanations to the competent authors dealing with the very complex atmospheric events that occur in the Indian Ocean and its environs, were it not for the fact that two workers at least have expressed doubts about the classical theory and have published studies that reinforce those doubts (Sawyer 1947, Solot 1943). The effect of Solot's and Sawyer's papers is as follows :

Temperature contrasts exist at sharp boundaries in northwest India and in eastern Africa and they can be marked on maps as fronts. In understanding the genesis and development of perturbations of the summer monsoon, however, the temperature-contrast lines occur in positions and undergo variations in space and time that make it very difficult to attribute to them the role that extratropical fronts play in high-latitude cyclones. A cyclonic shear in the winds, for example, often does not occur at a ' front ' but at considerable distances from it, in an air mass thermodynamically homogeneous. Moreover, at a long-lived front, there is almost invariably an easily-detected horizontal velocity convergence throughout the lower atmosphere, and the correlation with heavy cloud and precipitation is not with the ' front ' as such but with convergence taking place in a moist air mass. The usefulness of the methods of air-mass analysis in the regions considered, then, is not so much in establishing the dynamic basis for the occlusion process and a re-distribution of mass in the perturbation, but in identifying the origin of the air that converges along asymptotes. In the absence of sufficient winds for a detailed kinematic analysis which might lead to the computation of horizontal velocity divergence and vertical motion and to identification of kinematic models, waves and vortices, the air-mass analysis provides a means of reconstructing the trajectories of the air parcels passing over the area of interest and consequently, by inference and in conjunction with such winds as are available, of the streamline patterns. In continental regions and in the absence of dense rawinsonde networks, the air-mass technique might well be modified to make this aim explicit – to find the position and estimate the transformations of the major asymptotes and singularities of the field of motion and to link them dynamically with the pressure and humidity fields.

The point at issue is theoretical. From oceanic tropical meteorology we know that temperature differences across asymptotes of convergence play at most a secondary part in the instability of waves in the tropical atmosphere. The most violent storms known to synoptic meteorologists (excluding the tornado as a local storm akin to the thunderstorm) is the tropical cyclone. It develops in an air-mass that is, within the limits of observational error, homogeneous. Temperature contrasts at asymptotes exist over the continents, but it is difficult to believe that they play the major role in the genesis of monsoon perturbations. In this connection it should be emphasized that increasing attention is being paid by leaders in high-latitude meteorology to the dynamics of major zonal currents, of the long-wave perturbations in them and of instability that is independent of the existence of frontal discontinuities (Bjerknes 1937, Rossby 1942, Holmboe 1948, Höiland 1941, Sutcliffe 1939, 1951). The model atmospheres that these authors contemplate are in some cases barotopic, in others baroclinic. But in the baroclinic cases, the instability they are investigating does not necessarily depend upon the concentration of the solenoids at a discontinuity. It would be unfortunate if tropical meteorologists, who have the greatest opportunties to contribute to the new investigations in the empirical field, should finally appear as a die-hard group which was either ignorant or oblivious of the great changes that the original air-mass theory has undergone in the past twenty years.

Acknowledgments

Since their work is so little known outside the United States it seems appropriate to mention and thank workers in or associated with the Tropical Pacific Project, University of California : L. Sherman, G. Dean, H. Landers, Captain O. W. Stopinski,

U.S.A.F. and Captain F. R. Miller, U.S.A.F., whose contributions to Pacific meteorology have clarified the views presented here. My thanks are also due to Professor H. Riehl for discussion of several points in connection with double vortices.

The work of the Tropical Project has been made possible through support and sponsorship extended by the Geophysics Research Division of the U.S.A.F. Cambridge Research Center under Contract No. W28-099ac-385.

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551.521.3

A statistical model for water-vapour absorption

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(Manuscript received 19 December 1951, in revised form 22 January 1952)

SUMMARY

A statistical model of a water-vapour absorption band is discussed, and comparison with computations made by Cowling shows that it provides a very accurate account of the average transmission over narrow frequency bands. The model can be used to compute the atmospheric transmission by water vapour under conditions of meteorological importance which were not treated by Cowling.

1. INTRODUCTION

Cowling (1950) has computed the run of absorption in the water-vapour rotation band starting with the detailed theoretical structure of the band and the Lorentz line shape. Since he leaves the mean line intensity and the line width as variables his computations are of great value despite some doubt as to the precise values of these two parameters. As a result a set of data now exists against which it is possible to test the accuracy of any simplified models, and if, in addition, the model is physically plausible then good agreement with Cowling's data will indicate that it may safely be applied to cases not treated by him. Such cases are :

(a) Transmission of a stream of diffuse radiation.
(b) Paths along which pressure and concentration vary together.
(c) Mixed Doppler and Lorentz line shape (for levels above 30 km).
(d) Transmission by the atmospheric window near 10 μ.

In this paper the statistical model will only be discussed in relation to Cowling's data in order to demonstrate its precision and simplicity ; the other cases mentioned above will be discussed in later papers.

The symbols used in this paper correspond to those used by Goody and Robinson (1951).

2. THE RANDOM MODEL

Cowling's computations were made for six ranges of wave length and he was led to the conclusion that ' in atmospheric work complication is avoided, and remarkably little error is involved, if a single absorption curve is used at all wave lengths.' Inspection of the diagram of line intensities given by Randall, Dennison, Ginsberg and Weber (1937) suggests that the only common feature of these ranges is a nearly random distribution of line intensities and positions. Although it is known that these are connected by quantum-mechanical formulae, Cowling's empirical observation suggests that in practice it may be sufficient to consider a random model.

Consider the transmission in the centre of a band of frequencies of width $n\delta$, where n is the number of absorption lines and δ the mean line spacing. Let $n\delta$ be so large that the influence at the band centre of lines outside the band may be neglected. Postulating no correlation between line positions and intensities, the transmission can now be specified in terms of $N(\nu_1 \ldots \nu_n)\, d\nu_1 \ldots d\nu_n$ and $P(k)dk$, where the former

is the probability that lines occur in the frequency ranges ν_1 to $\nu_1 + d\nu_1$ etc., and the latter is the probability that one line has an intensity in the range k to $k + dk$ (see Goody and Robinson (1951) for definitions.)

Let the absorption coefficient at a distance ν_r from a line centre be $k_r\, S(\nu_r, \alpha)$ where S is a line-shape parameter and α is the line width. The transmission of m g cm^{-2} at the band centre caused by a line whose frequency is displaced by ν_r is

$$\exp\left[-m\, k_r\, S(\nu_r, \alpha)\right].$$

The probability of a set of n lines is given by

$$N(\nu_1 \ldots \nu_n)\, d\nu_1 \ldots d\nu_n \prod_1^n P(k_r)\, dk_r,$$

and therefore the mean transmission for all possible arrangements is

$$\bar{T}(\alpha, m) =$$

$$= \frac{\int_{-n\delta/2}^{+n\delta/2} \ldots \int_{-n\delta/2}^{+n\delta/2} d\nu_1 \ldots d\nu_n\, N(\nu_1 \ldots \nu_n) \int_0^\infty \ldots \int_0^\infty \prod_1^n P(k_r) \exp\left[-mk_r\, S(\nu_r, \alpha)\right] dk_r}{\int_{-n\delta/2}^{+n\delta/2} \ldots \int_{-n\delta/2}^{+n\delta/2} d\nu_1 \ldots d\nu_n\, N(\nu_1 \ldots \nu_n) \int_0^\infty \ldots \int_0^\infty \prod_1^n P(k_r)\, dk_r} \quad . \quad . \quad (1)$$

If it is now postulated that all arrangements of line positions are equally probable, then N is simply a constant and may be cancelled out. All the frequency and all the intensity integrations are now the same and, with slight re-arrangement, Eq. (1) may be written,

$$\bar{T}(\alpha, m) = \left\{1 - \frac{1}{n\delta} \int_{-n\delta/2}^{+n\delta/2} \int_0^\infty P(k)\left(1 - \exp\left[-mk\, S(\nu, \alpha)\right]\right) dk\, d\nu \Big/ \int_0^\infty P(k)\, dk\right\}^n,$$

which, in the limit of large n, tends to

$$\bar{T}(\alpha, m) = \exp\left[-\frac{1}{\delta} \int_{-\infty}^{+\infty} \int_0^\infty P(k)\left(1 - \exp\left[-mk\, S(\nu, \alpha)\right]\right) dk\, d\nu \Big/ \int_0^\infty P(k)\, dk\right] \quad . \quad (2)$$

In order to integrate this expression $P(k)$ must be known. Two forms have been tried,

$$P(k) = \frac{1}{\sigma} \exp\left(-k/\sigma\right)$$

and a delta function which implies that all the lines have the same intensity. By choosing parameters both of these forms yield results which agree well with Cowling's data. The former gives slightly the better results and is a better representation of the known distribution of water-vapour line intensities ; it will therefore be used in this paper although it clearly does not matter within wide limits which distribution is chosen. It will be seen that the new parameter σ, which has been introduced, is simply the mean line strength.

In principle a whole series of transmission functions may be found by using different expressions for $P(k)$ and it is possible that, for other molecules with three differing moments of inertia (and therefore very complicated line spectra), some other expression will give the best fit to observations.

Substituting the exponential distribution in Eq. (2) the integration may be performed to give,

$$\bar{T}(\alpha, m\sigma) = \exp\left[-\frac{1}{\delta}\int_{-\infty}^{+\infty} \frac{m\sigma\, S(\alpha, \nu)}{1 + m\sigma\, S(\alpha, \nu)}\, d\nu\right] \quad . \quad . \quad (3)$$

Eq. (3) reduces the problem of a complicated absorption band to an integral involving the shape of a single line, leading to an immense simplification in numerical work. Since this paper is only concerned with a comparison with Cowling's work, the Lorentz shape

$$S(\nu, \alpha) = \frac{\alpha/\pi}{\nu^2 + \alpha^2}$$

may be substituted to give

$$\bar{T}(\alpha, m\sigma) = \exp\left[-\frac{m\sigma\alpha}{\delta\left(\alpha^2 + \frac{m\sigma\alpha}{\pi}\right)^{\frac{1}{2}}}\right] \quad . \quad . \quad . \quad (4)$$

3. Comparison with Cowling's data

In Fig. 1 the expression in Eq. (4) is compared with the computations for the water-vapour rotation band. In each case the values of α are the same but the values of σ and δ are chosen for the best fit.

The agreement in Fig. 1 is remarkably good. The best agreement would be expected with the 'universal' curve which represents an average over many lines (a). The range 125 cm^{-1} to 100 cm^{-1} contains only 17 lines, but this number appears to be sufficiently large for good agreement with a curve based upon statistical arguments (b). Since this range was computed for 4 pressures it is also possible to demonstrate that Eq. (4) gives a very precise account of the influence of pressure upon absorption. (c) is an average over three of Cowling's ranges since each individual range contained very few lines. Similarly (d) is an average of two ranges which together contain only 14 lines. The poor agreement for transmissions less than 0·15 indicates that the range of averaging of 50 cm^{-1} is too small for a good comparison in this region of the spectrum.

The agreement exhibited in Fig. 1 shows that the random model, already physically satisfying, is numerically precise to a degree far beyond the requirements of atmospheric calculations. It may therefore be used with confidence under conditions which have not already been computed.

4. Data for computations

Cowling has stated that his 'universal' curve may, with adequate accuracy, be used for all frequency ranges in the water-vapour absorption spectrum. Apart from the translucent region from 8 to 15 μ this is probably correct, and to the same degree of accuracy Eq. (4) may be used with $\delta = 2{\cdot}00\ cm^{-1}$ and $\log_{10}\sigma = 0{\cdot}705 - \log_{10} m_0$, where m_0 is from Fig. 1(a) the amount of water vapour required to give a transmission of $\frac{1}{2}$. m_0 has been tabulated by Cowling (his Table II) from 5 to 50 μ. The only modification to Cowling's data which seems worth while is the use of $\alpha = 0{\cdot}10\ cm^{-1}$ at one-atmosphere pressure in place of the value 'rather above $2\alpha = 0{\cdot}25\ cm^{-1}$' which he suggests; the former is more in accord with the experiments of Adel (1947) and Becker and Autler (1946).

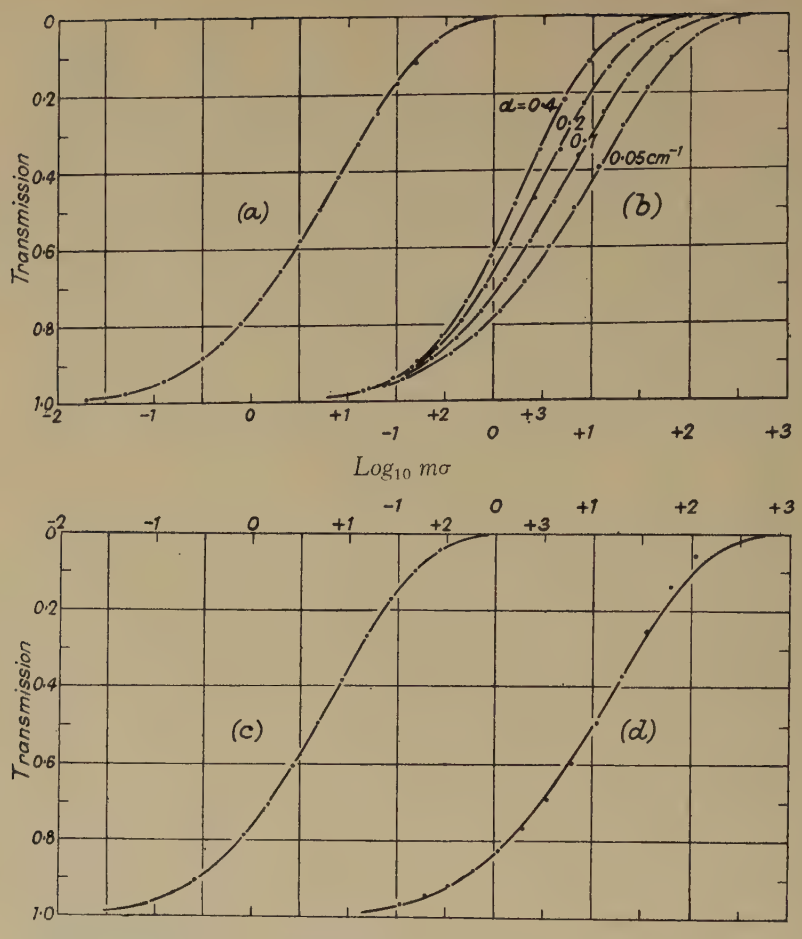

Figure 1. Comparison between the random model and Cowling's data. The points are Cowling's and the line is given by Eq. (4).

(a) Cowling's ' universal ' curve, $\alpha = 0.13\ \text{cm}^{-1}$, $\delta = 2.00\ \text{cm}^{-1}$ and $\sigma = 5.01/m_0$ where m_0 is the amount of water required to give a transmission of 1/2.
(b) $125\ \text{cm}^{-1}$ to $100\ \text{cm}^{-1}$, $\alpha = 0{\cdot}4\ \text{cm}^{-1}$, $0{\cdot}2\ \text{cm}^{-1}$, $0{\cdot}1\ \text{cm}^{-1}$, and $0{\cdot}05\ \text{cm}^{-1}$, $\delta = 1{\cdot}48\ \text{cm}^{-1}$ and $\sigma = 4{\cdot}52 \times 10^3\ \text{cm}^2\ \text{g}^{-1}$.
(c) $175\ \text{cm}^{-1}$ to $250\ \text{cm}^{-1}$, $\alpha = 0{\cdot}2\ \text{cm}^{-1}$, $\delta = 2{\cdot}27\ \text{cm}^{-1}$ and $\sigma = 8{\cdot}00 \times 10^3\ \text{cm}^2\ \text{g}^{-1}$.
(d) $300\ \text{cm}^{-1}$ to $350\ \text{cm}^{-1}$, $\alpha = 0{\cdot}2\ \text{cm}^{-1}$, $\delta = 3{\cdot}5\ \text{cm}^{-1}$ and $\sigma = 3{\cdot}33 \times 10^3\ \text{cm}^2\ \text{g}^{-1}$.

From fitting the curves in Fig. 1 three individual values of δ and σ have been obtained. On the basis of these three points an attempt has been made to construct curves which show the run of these parameters through the rotation band in case a more accurate representation than the ' universal ' curve should ever be required.

The three points available are given by the crosses in Fig. 2 (a) and (b) and the recommended curves are given by the full lines. The ordinate used in (a) is the mean absorption coefficent rather than the mean line intensity ; this quantity is chosen, since for large transmissions it is the only important one.

The recommended curves have been drawn to pass through the three points available, using an analogue as a guide to the general shape. In the case of (a) the analogue has been drawn upon the assumption that Cowling's ' universal ' curve and

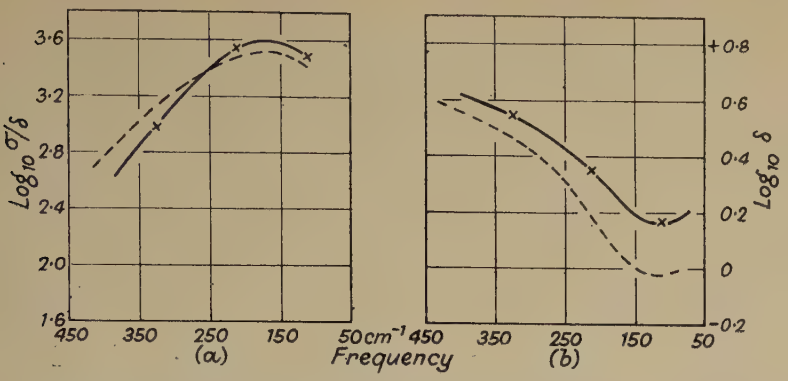

Figure 2. Mean line spacing and intensities for the rotation band. The full line in Fig. 2 is the recommended curve. The points correspond to the three ranges in Fig. 1.

(a) The dashed line is obtained by assuming that Cowling's data are correct at a transmission of $\frac{1}{2}$.
(b) The dashed line gives the mean spacing of absorption lines in the tables of Randall *et al.*

his table for m_0 are correct for a transmission of $\frac{1}{2}$ and, further, that the values of δ given by (b) are correct.

In the case of (b) the analogue is simply the mean spacing of lines given in the tables of Randall *et al.* and the difference between this and the recommended curve arises since the exponential distribution predicts too few weak lines.

Acknowledgments

The writer wishes to thank the Royal Commission of the 1851 Exhibition for a Senior Studentship which has been held while this work was in progress, and Prof. T. G. Cowling, F.R.S., for providing details of his computations.

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551.515.8

Dynamical aspects of some simple frontal models

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(Manuscript received 21 August 1951)

SUMMARY

The dynamics of the motion at an idealized frontal surface are considered theoretically. For this purpose the front is treated as a simple discontinuity of density, but it is assumed that horizontal and vertical gradients of temperature and wind may exist within the air masses. It is shown that at a front of this character between two non-homogeneous air masses there must be a discontinuity of vertical velocity at the frontal surface, i.e., there must be up-sliding or down-sliding. The discontinuity in vertical velocity is related to the gradients of wind in the two air masses and is shown to be of a similar magnitude (or only slightly smaller) than the vertical velocities believed to occur in frontal regions.

1. INTRODUCTION

The idealized model of a front which is presented in the standard text-books (Brunt 1941) consists of two air masses each with uniform temperature and velocity separated by a sloping surface of discontinuity. If such a front is stationary, steady horizontal motion is possible in both air masses. If the front is to be moving the model does not contain any explanation of the pressure changes which must take place, but, even if we postulate the necessary small convergence and divergence in the air masses themselves, the existence of the discontinuity still provides no explanation of the movement of air through the isobaric surfaces, which must occur in order to produce the characteristic frontal cloud and rainfall.

R. C. Sutcliffe (1947) has developed a treatment of the motion of an atmosphere without discontinuities which has considerable success in explaining anticyclonic and cyclonic development and the associated vertical motions. Broadly, development and vertical motion is expected in regions where at some level there is a change of the vertical component of vorticity along the direction of the vertical shear. At an ideal front the wind shear is parallel to the front ; the vorticity is zero in the two air masses and infinite on the frontal surface which is a vortex sheet of uniform intensity. The simple front is thus the limiting case of non-developing baroclinic field, and we are led to study more complex models in order to seek the explanations of the vertical movements at fronts.

We shall, therefore, consider the motion at a frontal surface between two air masses which are not assumed to be homogeneous. Horizontal and vertical gradients of temperature and wind are assumed to exist in both air masses in addition to the discontinuity at the frontal surface.

As is well known the slope of a front is directly related to the geostrophic wind speeds and temperatures in the two air masses. On the other hand changes in the slope of the front must conform with the kinematics of the motion. These two requirements enable one to derive a relation between the rate of change of the wind speeds parallel to the front and the existing wind and temperature field in the two air masses and thereby to obtain the accelerations of the air parallel to the front. These accelerations are related to the ageostrophic component perpendicular to the front and thence, from considerations of continuity, to the vertical movement at the front. It will be

shown that at a front between non-homogeneous air masses relative vertical motion of the air masses will occur.

In order to consider the vertical motion at fronts, it is necessary to consider the ageostrophic component of the motion. This is approached by relating the ageostrophic component to the acceleration of the air and it will be assumed that this acceleration can be adequately determined from the *geostrophic* wind field and its changes. This is a device which has already been recommended by Charney (1948) and used by Sutcliffe (1947) in obtaining valuable approximate solutions to the equations of atmospheric flow. No further justification will be attempted here.

2. Coordinates and notation

In the following treatment rectangular isobaric coordinates will be used with horizontal coordinates x, y, but the pressure coordinate, p, will be replaced by a coordinate Z defined by the equation

$$Z = -\frac{RT_0}{g}\log p \qquad . \quad . \quad . \quad . \quad . \quad (1)$$

in which R is the gas constant, g is the acceleration due to gravity and T_0 is a standard temperature, conveniently chosen as the temperature at 1,000 mb.

The hydrostatic relation is

$$\partial p/\partial z = -g\rho \qquad . \quad . \quad . \quad . \quad . \quad (2)$$

where z is the height above a standard level and ρ the density.

From Eqs. (1) and (2) it readily follows that

$$\delta Z = \frac{T_0}{T}\delta z \qquad . \quad . \quad . \quad . \quad . \quad (3)$$

where T is the temperature at the level concerned.

Z is thus a vertical coordinate with dimensions of length and roughly equal to the true height.

The following symbols will also be used :

h = height of an isobaric surface

l = Coriolis parameter

u, v = horizontal components of velocity in the x, y directions

U, V = geostrophic wind components in the x, y directions, so that

$$U = -\frac{g}{l}\frac{\partial h}{\partial y}, \qquad V = \frac{g}{l}\frac{\partial h}{\partial x} \qquad . \quad . \quad . \quad (4)$$

U_f = velocity of the intersection of the front with the x-axis

d/dt = differentiation following the motion of the fluid

$\frac{D}{Dt} = \frac{\partial}{\partial t} + U_f\frac{\partial}{\partial x}$, differentiation following the motion of the front

$\varpi = dZ/dt$, approximately equal to the vertical component of velocity

$\alpha = (\partial Z/\partial x)_{yf}$, the differentiation following the intersection of the front with the x, z plane; α is very closely equal to the slope of the front.

Γ = saturated or dry adiabatic lapse rate as appropriate to the condition of the air.

γ = lapse rate of temperature

$L, M, N,$ = direction cosines of normal to frontal surface in the x, y, Z space.

All differentiation with respect to x and y is at constant pressure (Z constant).

Suffices 1 and 2 will be used to distinguish the velocities, densities, temperatures, etc., in the two air masses. The symbol Δ will be used to denote the change in any variable at the front, e.g., $\Delta u = u_1 - u_2$ and a bar above the symbol will be used to denote the mean value of the variable between the two air masses, $\bar{u} = \frac{1}{2}(u_1 + u_2)$.

The axes will be chosen so that the y axis is parallel to the front at the origin initially. The front between two non-homogeneous air masses is not plane, and consequently the front will not necessarily be parallel to the y axis at other points than the origin, nor at subsequent times.

3. Equation for the slope of the front

At the front the height of any isobaric surface must be continuous. Thus

$$h_1(x, y, Z) = h_2(x, y, Z) \qquad (5)$$

This is the equation for the frontal surface. Differentiating Eq. (5), we have for any displacement $\delta x, \delta y, \delta Z$ in the frontal surface,

$$\left(\frac{\partial h_1}{\partial x} - \frac{\partial h_2}{\partial x}\right)\delta x + \left(\frac{\partial h_1}{\partial y} - \frac{\partial h_2}{\partial y}\right)\delta y + \left(\frac{\partial h_1}{\partial Z} - \frac{\partial h_2}{\partial Z}\right)\delta Z = 0 \qquad (6)$$

Since h is a height above a standard level, we have from Eq. (3)

$$\frac{\partial h}{\partial Z} = \frac{T}{T_0}$$

and substituting from this equation and the geostrophic wind Eq. (4) in Eq. (6) we obtain

$$\frac{l\,\Delta V}{g}\delta x - \frac{l\,\Delta U}{g}\delta y + \frac{\Delta T}{T_0}\delta Z = 0.$$

Whence it follows that

$$\alpha = \left(\frac{\partial Z}{\partial x}\right)_{yf} = -\frac{l\,T_0\,\Delta V}{g\,\Delta T} \qquad (7)$$

and the direction cosines L, M, N of the normal to the front are given by

$$\frac{gL}{l\,\Delta V} = -\frac{gM}{l\,\Delta U} = \frac{NT_0}{\Delta T} \qquad (8)$$

Differentiation of Eq. (7) following the motion of the front gives a relation for the rate of change of the slope of the front, viz :

$$l\,\Delta V \frac{D}{Dt}\left(\frac{1}{\alpha}\right) + \frac{l}{\alpha}\frac{D}{Dt}(\Delta V) + \frac{g}{T_0}\frac{D}{Dt}(\Delta T) = 0 \qquad (9)$$

In the following paragraphs independent relations for

$$\frac{D}{Dt}\left(\frac{1}{\alpha}\right), \quad \frac{D}{Dt}(\Delta V) \quad \text{and} \quad \frac{D}{Dt}(\Delta T)$$

will be derived on kinematical and dynamical grounds. These will be substituted into Eq. (9) giving finally a relation between the discontinuity in vertical velocity, $\Delta\varpi$, and other characteristics of the front.

4. The equation of continuity

An equation expressing the requirement of continuity can be obtained from the fact that the relative motion of adjacent particles of the two air masses must be parallel to the front. This is also true in the (x, y, Z) space.

Therefore $L\,\Delta u + M\,\Delta v + N\,\Delta\varpi = 0$

and at the origin

$$\alpha\,\Delta u = \Delta\varpi \qquad . \qquad . \qquad . \qquad . \qquad . \qquad . \qquad . \qquad (10)$$

5. The dynamical condition

The equations of motion of both air masses in the y direction have the form

$$\frac{\partial v}{\partial t} + u\frac{\partial v}{\partial x} + v\frac{\partial v}{\partial y} + \varpi\frac{\partial v}{\partial Z} = -l\,(u - U) \qquad . \qquad . \qquad . \qquad (11)$$

In this equation we shall regard the ageostrophic components of the wind as small compared with the geostrophic components. The difference between the geostrophic wind component and the motion of the front is of the same order of magnitude as the ageostrophic wind, i.e., $u - U_f$ can be neglected compared with U. On the other hand in Eq. (11), $\frac{1}{u}\frac{\partial v}{\partial t}$, $\frac{\partial v}{\partial x}$ etc. are all considerably smaller than l so that the term on the right is of a similar magnitude to those on the left. We shall therefore assume that Eq. (11) will give an approximate value for the geostrophic departure when we have substituted $v = V$ and $u = U_f$ on the left hand side obtaining,

$$\frac{DV}{Dt} + V\frac{\partial V}{\partial y} + \varpi\frac{\partial V}{\partial Z} = -l\,(u - U) \qquad . \qquad . \qquad . \qquad (12)$$

Applying Eq. (12) to the two air masses and subtracting, we obtain at the origin, where $U_1 = U_2$,

$$\frac{D}{Dt}(\Delta V) + \frac{1}{2}\frac{\partial}{\partial y}\Delta(V^2) + \overline{\varpi}\,\Delta\left(\frac{\partial V}{\partial Z}\right) + \Delta\varpi\,\overline{\frac{\partial V}{\partial Z}} = -l\,\Delta u \qquad . \qquad (13)$$

and substituting from Eq. (10) at the origin

$$\frac{D}{Dt}(\Delta V) = -\frac{1}{2}\frac{\partial}{\partial y}\Delta(V^2) - \overline{\varpi}\,\Delta\left(\frac{\partial V}{\partial Z}\right) - \Delta\varpi\,\overline{\frac{\partial V}{\partial Z}} - \frac{l}{\alpha}\Delta\varpi \qquad . \qquad (14)$$

6. Kinematic conditions

The velocity of the front in the direction of the normal in the x, y, Z space is equal to the velocity of the adjacent air of one air mass in this direction, namely :

$$u_1 L + v_1 M + \varpi_1 N .$$

Consequently the velocity U_f, of the intersection of the front with the x axis, is given by

$$U_f = u_1 + v_1 \frac{M}{L} + \varpi_1 \frac{N}{L}.$$

If now we consider the variation of U_f with height along the intersection of the front with the x, Z plane, we obtain an expression for the rate of change of the slope of the front from kinematical considerations.

We denote differentiation along the frontal surface in the x, Z plane by

$$\left(\frac{\partial}{\partial Z}\right)_f = \frac{\partial}{\partial Z} + \frac{1}{\alpha}\frac{\partial}{\partial x}.$$

Then

$$\frac{D}{Dt}\left(\frac{1}{\alpha}\right) = \left(\frac{\partial U_f}{\partial Z}\right)_f$$

$$= \left(\frac{\partial U_1}{\partial Z}\right)_f + \left(\frac{\partial v_1}{\partial Z}\right)_f \frac{M}{L} + v_1 \left\{\frac{\partial}{\partial Z}\left(\frac{M}{L}\right)\right\}_f - \frac{1}{\alpha}\left(\frac{\partial \varpi_1}{\partial Z}\right)_f - \varpi_1 \left\{\frac{\partial}{\partial Z}\left(\frac{1}{\alpha}\right)\right\}_f \quad . \quad (15)$$

At the origin $M = 0$ from our choice of axes ; also $M/L = -(\partial x/\partial y)_{Zf}$, the differentiation being along the frontal surface with Z constant.
Therefore

$$\left\{\frac{\partial}{\partial Z}\left(\frac{M}{L}\right)\right\}_f = -\frac{\partial}{\partial y_f}\left(\frac{\partial x}{\partial Z}\right)_{fy} = -\frac{\partial}{\partial y}\left(\frac{1}{\alpha}\right).$$

If we substitute from these relations in Eq. (15) and approximate by means of the geostrophic wind relation we have

$$\frac{D}{Dt}\left(\frac{1}{\alpha}\right) = \frac{\partial U_1}{\partial Z} + \frac{1}{\alpha}\frac{\partial U_1}{\partial x} - V_1 \frac{\partial}{\partial y}\left(\frac{1}{\alpha}\right) - \frac{1}{\alpha}\left(\frac{\partial \varpi_1}{\partial Z}\right) - \frac{1}{\alpha^2}\left(\frac{\partial \varpi_1}{\partial x}\right) - \varpi_1 \frac{\partial}{\partial Z_f}\left(\frac{1}{\alpha}\right) \quad . \quad . \quad (16)$$

Combining Eq. (16) with a similar equation for the second air mass and making use of the relation $\partial U/\partial x = -\partial V/\partial y$, we obtain

$$\frac{D}{Dt}\left(\frac{1}{\alpha}\right) = \overline{\frac{\partial U}{\partial Z}} - \overline{\frac{\partial}{\partial y}\left(\frac{V}{\alpha}\right)} - \frac{1}{\alpha}\overline{\left(\frac{\partial \varpi}{\partial Z}\right)} - \frac{1}{\alpha^2}\overline{\left(\frac{\partial \varpi}{\partial x}\right)} - \bar{\varpi}\frac{\partial}{\partial Z_f}\left(\frac{1}{\alpha}\right) \quad . \quad . \quad . \quad . \quad (17)$$

7. The thermodynamic condition

If we treat the motion as adiabatic,

$$\frac{dT}{dt} = \frac{dT}{dp}\frac{dp}{dt} = -\frac{gp\varpi}{RT_0}\frac{dT}{dp},$$

but the appropriate dry or saturated adiabatic lapse rate, Γ, is given by $\frac{gp}{RT}\frac{dT}{dp}$.
Therefore

$$\frac{dT}{dt} \equiv \frac{\partial T}{\partial t} + u\frac{\partial T}{\partial x} + v\frac{\partial T}{\partial y} + \varpi\frac{\partial T}{\partial Z} = -\varpi\frac{T}{T_0}\Gamma.$$

Thus writing γ for the lapse rates in the two air masses and approximating by means of the geostrophic condition and the approximate relation $u \simeq U_f$ we obtain

$$\frac{D}{Dt}(\Delta T) = -\overline{V}\Delta\left(\frac{\partial T}{\partial y}\right) - \Delta V\frac{\overline{\partial T}}{\partial y}$$
$$-\frac{\overline{\varpi}\,\overline{T}\,\overline{\Gamma-\gamma}}{T_0}\left(\frac{-\Delta\gamma}{\overline{\Gamma-\gamma}} + \frac{\Delta T}{\overline{T}} + \frac{\Delta\varpi}{\overline{\varpi}} - \frac{1}{4}\frac{\Delta\gamma}{\overline{\Gamma-\gamma}}\frac{\Delta T}{\overline{T}}\frac{\Delta\varpi}{\overline{\varpi}}\right) \quad . \quad . \quad . \quad (18)$$

The thermal wind relation may be obtained by differentiating the geostrophic wind condition with respect to Z thus :

$$\frac{\partial U}{\partial Z} = -\frac{g}{l}\frac{\partial}{\partial Z}\left(\frac{\partial h}{\partial y}\right) = -\frac{g}{l}\frac{\partial}{\partial y}\left(\frac{\partial h}{\partial Z}\right) = -\frac{g}{l}\frac{\partial}{\partial y}\left(\frac{T}{T_0}\right) = -\frac{g}{lT_0}\frac{\partial T}{\partial y} \quad . \quad . \quad . \quad . \quad (19)$$

Substituting from (19) in (18)

$$\frac{D}{Dt}(\Delta T) = \frac{lT_0}{g}\overline{V}\Delta\frac{\partial U}{\partial Z} + \frac{lT_0}{g}\Delta V\frac{\overline{\partial U}}{\partial Z}$$
$$-\frac{\overline{\varpi}\,\overline{T}\,\overline{\Gamma-\gamma}}{T_0}\left(-\frac{\Delta\gamma}{\overline{\Gamma-\gamma}} + \frac{\Delta T}{\overline{T}} + \frac{\Delta\varpi}{\overline{\varpi}} - \frac{1}{4}\frac{\Delta\gamma}{\overline{\Gamma-\gamma}}\frac{\Delta T}{\overline{T}}\frac{\Delta\varpi}{\overline{\varpi}}\right) \quad . \quad . \quad . \quad (20)$$

8. The relative vertical velocities at the front

Equations (14), (17) and (20) give the rates of change following the motion of the front of $\Delta\,(1/\alpha)$, ΔV and ΔT. However, these rates of change must satisfy Eq. (9) which relates them and which was derived from the usual equation for the slope of the front. Substitution from Eqs. (14), (17) and (20) in Eq. (9) gives an equation which can be simplified by means of the thermal wind Eq. (19) and Eq. (7) for the slope of the front to become

$$\Delta\varpi + \Delta V\frac{\alpha^2}{l}\frac{\partial}{\partial Z_f}\left(\frac{\overline{\varpi}}{\alpha}\right) = -2\frac{\alpha}{l}\Delta V\frac{\partial\overline{V}}{\partial y} + 2\frac{\alpha^2}{l}\Delta V\frac{\overline{\partial U}}{\partial Z} - \frac{\alpha}{l}\left\{\overline{\varpi}\,\Delta\left(\frac{\partial V}{\partial Z}\right) + \Delta\varpi\left(\frac{\overline{\partial V}}{\partial Z}\right)\right\}$$
$$-\frac{g\alpha^2\,\overline{\varpi}\,\overline{T}\,\overline{\Gamma-\gamma}}{l^2\,T_0^2}\left\{-\frac{\Delta\gamma}{\overline{\Gamma-\gamma}} + \frac{\Delta T}{\overline{T}} + \frac{\Delta\varpi}{\overline{\varpi}} - \frac{1}{4}\frac{\Delta V}{\overline{\Gamma-\gamma}}\frac{\Delta T}{\overline{T}}\frac{\Delta\varpi}{\overline{\varpi}}\right\} \quad (21)$$

This equation expresses the discontinuity in vertical motion $\Delta\varpi$ in terms of other factors. All the terms on the right hand side of Eq. (18) may be made to vanish separately by specifying that the air masses shall have specific characteristics of motion or specified properties. For example the first term vanishes at a front at which $\partial\overline{V}/\partial y = 0$ (i.e., the average component of motion along the front does not change in that direction). It is therefore convenient to discuss each term separately.

The second term on the left-hand side of Eq. (21) depends primarily on the vertical velocities themselves. However, we can form an estimate of its importance by considering a simple case in which the change of vertical velocity at the front, $\Delta\varpi$, is the same at all levels, and in which the vertical velocity of the cold air vanishes at the ground and of the warm air at the tropopause. If the changes of vertical velocity with height are approximately linear and α is independent of height then $\frac{\partial}{\partial Z_f}\left(\frac{\overline{\varpi}}{\alpha}\right) \simeq \frac{\Delta\varpi}{\alpha Z_T}$ where Z_T is the value of Z at the tropopause. If we insert typical values viz : $\Delta V = 20$ mi hr^{-1}, $l = 0{\cdot}4$ hr^{-1}, $Z_T = 6$ mi and $\alpha = 0{\cdot}01$ we find that under these circumstances the term $\frac{\alpha^2}{l}\Delta V\frac{\partial}{\partial Z_f}\left(\frac{\overline{\varpi}}{\alpha}\right) = \frac{\Delta\varpi}{12}$, so that it is small compared with the preceding term $\Delta\varpi$.

We may therefore ignore the second term on the left of Eq. (21) with confidence that it will not seriously affect the general character and magnitude of the results derived regarding $\Delta\varpi$.

9. The effect of the change of wind field along the front

The first term on the right of Eq. (21), $-2\frac{\alpha}{l}\Delta V\frac{\partial\overline{V}}{\partial y}$, vanishes if the wind component along the front does not change as we proceed along the front. If the x axis is directed towards the cold air and suffix 1 refers to the warm air mass, then $\Delta V = V_1 - V_2 < 0$. Thus this term makes a positive contribution to $\Delta\varpi$ when $\partial/\partial y\,(V_1 + V_2) > 0$. Thus the warm air moves upward relative to the cold air if the mean component velocity of the two air masses along the front away from an observer increases as distance from him increases, i.e., in a frontogenetic wind field.

To determine the magnitude of the effect we may insert typical values, namely,

$\alpha = 0{\cdot}01$, $l = 0{\cdot}4\ \text{hr}^{-1}$, $\Delta V = 20\ \text{mi hr}^{-1}$, $\partial\overline{V}/\partial y = 20\ \text{mi hr}^{-1}$ per 200 mi $= 0{\cdot}1\ \text{hr}^{-1}$

Then $\Delta\varpi = 0{\cdot}1\ \text{mi hr}^{-1} = 5{\cdot}2\ \text{cm sec}^{-1}$.

This is rather smaller than the total upward velocity at some fronts, which may exceed 10 cm sec^{-1}, but it is of broadly the correct order of magnitude.

10. The effect of a vertical wind shear directed across the front

The second term on the right-hand side of Eq. (21) vanishes if the thermal wind component in the two air masses has no component across the front, i.e., $\partial U_1/\partial Z = \partial U_2/\partial Z = 0$. If, however, there is such a component and it is directed from warm to cold air, that is 'up the front,' this term provides a negative contribution to $\Delta\varpi$ and the warm air moves downward relative to the cold air.

If we insert typical magnitudes such as

$$\alpha = 0{\cdot}01,\ l = 0{\cdot}4\ \text{hr}^{-1},\ \Delta V = 20\ \text{mi hr}^{-1},$$

$$\frac{\partial V_1}{\partial Z} = \frac{\partial V_2}{\partial Z} = 20\ \text{mi hr}^{-1}/4\ \text{mi (20 mi hr}^{-1}\ \text{in 20,000 ft)},$$

then $$2\frac{\alpha^2}{l}\Delta V\left(\frac{\partial U}{\partial Z}\right) = 0{\cdot}1\ \text{mi hr}^{-1} = 5{\cdot}2\ \text{cm sec}^{-1}.$$

This is a contribution to the vertical motion of the same order of magnitude as that arising from the first term. With the particular values chosen, the two terms are equal but with other equally probable values either of the terms might be the greater.

11. The effect of vertical wind shear parallel to the front

The third term on the right-hand side of Eq. (21) depends on the vertical wind shear parallel to the front, but also on the vertical motion itself. The term contains two components, the first depending on the difference of the vertical shear between the two air masses and the second on the mean vertical shear.

By inserting typical values the contribution of the first component is seen to be rather smaller than that of the previous terms.

Assume $\overline{\varpi} = 0{\cdot}1$ mi hr^{-1} (5 cm sec^{-1} approximately), $\alpha = 0{\cdot}01$, $l = 0{\cdot}4$ hr^{-1},

$$\Delta \frac{\partial V}{\partial Z} = 20 \text{ mi hr}^{-1} \text{ in 4 mi (20,000 ft approximately).}$$

Then $$\frac{\alpha}{l} \overline{\varpi}\, \Delta \left(\frac{\partial V}{\partial Z}\right) = \frac{1}{80} \text{ mi hr}^{-1} = 0{\cdot}6 \text{ cm sec}^{-1}.$$

The effect of the second component is best assessed by transferring it to the left-hand side of Eq. (21) and combining it with the first term, $\nabla\varpi$, which then becomes $(1 + k)\,\Delta\varpi$,

where $$k = \frac{\alpha}{l} \frac{\partial V}{\partial Z}.$$

If $\partial V/\partial Z > 0$, k > 0 and the effect is to reduce the value of $\Delta\varpi$ compared with the case of no vertical wind shear. Normally however $\partial V/\partial Z < 0$ and the effect of this term will be to increase the relative vertical velocities.

With $l = 0{\cdot}4$ hr^{-1}, $\alpha = 0{\cdot}01$, $\partial V/\partial Z = 5$ hr^{-1} we have $k = \frac{1}{8}$ so that the effect of the wind shear within the air masses of 20 kt in 20,000 ft is to change the relative vertical velocities by a factor of $\frac{1}{8}$ only.

12. The effect of vertical stability

The final term on the right-hand side of Eq. (21) represents the effect of vertical stability in the air masses. Within the bracket $\Delta T/\overline{T}$ is negligible compared with $\Delta\varpi/\overline{\varpi}$, and $\Delta\gamma/\overline{\Gamma - \gamma}$ is also usually smaller. We shall therefore consider the case in which the lapse rate is the same in both air masses, $\Delta\gamma = 0$, although the conclusions will be only slightly modified in the more general case, $\Delta\gamma \neq 0$.

The stability term then becomes $$-g \frac{\alpha^2}{l^2} \frac{\overline{T}}{T_0^2} (\Gamma - \gamma)\, \Delta\varpi.$$

Inserting typical values

$$\alpha = 0{\cdot}01, \quad l = 0{\cdot}4 \text{ hr}^{-1}, \quad T = T_0 = 280°\text{A},$$
$$\Gamma - \gamma = 6°\text{A mi}^{-1}, \quad g = 68{,}200 \text{ mi hr}^{-2}$$

we obtain

$$-g \frac{\alpha^2}{l^2} \frac{T}{T_0^2} (\Gamma - \gamma)\, \Delta\varpi \simeq -\Delta\varpi$$

Inserting this value in Eq. (18), it follows that the effect of vertical stability of this magnitude is to reduce the relative vertical velocity to about half its value at a front between air masses of neutral stability.

13. Conclusions

By considering an ideal sloping frontal discontinuity between two air masses it has been shown that a non-uniform distribution of temperature or motion within the air masses is necessarily accompanied by a vertical motion of one air mass relative to the other at the frontal surface, i.e., by upsliding or downsliding.

The main factors contributory to such upsliding or downsliding are :

(a) the variation along the front of the geostrophic wind component parallel to the front.

(b) the component of the thermal wind perpendicular to the front.

These factors operate in such a sense that :

(i) if the average component of velocity of the two air masses along the front increases along its own positive direction, then there will be a relative upward motion of the warm air.

(ii) that if the thermal wind is directed down the frontal slope then there will be a relative upward motion of the warm air.

The effect of thermal winds within the air masses parallel to the front is generally to increase slightly the computed relative vertical velocity but, on the other hand, vertical stability within air masses has the effect of reducing considerably the relative vertical velocity compared with that which would occur with air masses in neutral statical equibrium.

The values of the relative vertical velocity deduced, from typical values of the relevant gradients of velocity, frontal slope etc. are of somewhat smaller magnitude than the vertical velocities which are believed to occur in frontal regions. Nevertheless the deduced relative velocities are sufficient to have considerable influence on the structure and behaviour of fronts. It may be that the ideal frontal model studied here because of its mathematical convenience, does not adequately represent the complex structure of a front but it is not unlikely that relative movements of the warm and cold air masses do arise from essentially the causes described although superimposed on them are additional vertical movements resulting from larger-scale fields of convergence and divergence not entirely concentrated at the discontinuity surface itself.

Acknowledgments

I wish to acknowledge the assistance of Dr. R. C. Sutcliffe, Mr. F. H. Bushby and Mr. A. G. Matthewman in the preparation of this paper and in particular for the helpful suggestions which arose during discussion of the work. This paper is published with the permission of the Director of the Meteorological Office.

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551.511 : 551.551

The dissipation of kinetic energy in the lowest layers of the atmosphere

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(Manuscript received 23 October 1951, in revised form 17 January 1952)

Summary

The supply of kinetic energy to a shallow layer of air is considered. The cases chosen for analysis comprise occasions of comparatively strong wind and of fairly small temperature gradient so that the supply of kinetic energy from the vertical flux of total heat is not important at small heights. In these cases the diffusive flux of kinetic energy is also shown to be negligible. Hence it follows that, on these occasions, the rate of dissipation per unit volume is given by the product of the local values of the shearing stress and vertical gradient of mean velocity when steadiness and horizontal uniformity of the turbulence are assumed. Estimates of the rate of dissipation per unit mass at heights from 1 m to 50 m are made for particular occasions and used, together with the observed energy densities, to provide estimates of the size of the dissipating eddies at the heights of observation according to expressions given by L. F. Richardson and G. I. Taylor.

1. Introduction

Modern theories of turbulence agree in attaching importance to the rate of energy dissipation per unit mass of fluid (ϵ) as a parameter determining the characteristics of the flow. Although the theoretical use of this parameter demands its constancy over a period long compared with that of the largest eddy under investigation, little has been done towards estimating so much as its order of magnitude in the atmosphere and much less towards the determination of its spatial and temporal variation. Brunt (1939) has given an average value for the dissipation over the whole atmosphere as 5×10^7 erg sec^{-1} per square metre of the earth's surface, which is equivalent to an ϵ of 5 erg g^{-1} sec^{-1}. It is the aim of the present paper to form an estimate of ϵ on a particular occasion and at a particular place and it will be shown that, far from being of negligible effect as is commonly supposed, the dissipation of kinetic energy is an appreciable item in the energy balance of the lowest layers of the atmosphere.

It is as well to pause here to consider the significance of the word ' dissipation.' As usually taken, kinetic energy is dissipated when it is converted to heat by the action of viscous forces. After the conversion, the resultant thermal energy is distributed between the internal and potential energies of a particular mass of air but the manner of this distribution is quite irrelevant to the problem in hand. It is the actual conversion of kinetic into thermal energy by the action of molecular viscosity which constitutes dissipation. Neither the conversion of kinetic energy of mean flow into turbulent kinetic energy by the action of Reynolds's stresses nor the transfer of turbulent energy from eddies of one order to those of the next as visualized in similarity theories is, of itself, a process of dissipation in the present sense, although of course in the steady state the energy so converted or transferred is ultimately converted to thermal energy at the smallest eddy sizes and so ' dissipated.'

2. The energy balance of a layer of air

The turbulent-energy balance in a fluid of variable density has been discussed by Richardson (1920) and Calder (1949). For the present purpose, however, it is

more convenient to consider this balance in terms of the flux of kinetic energy into the top of a shallow layer of air bounded below by a uniform horizontal ground surface. This flux, F, is given by

$$F = \overline{\tfrac{1}{2}\rho\left\{(u_h + u')^2 + v'^2 + w'^2\right\}w'}$$

where ρ is the density of the air.
u_h is the mean wind velocity at height h.
u' is a fluctuation, in the direction of u_h.
v' is a horizontal velocity perpendicular to u_h.
w' is a fluctuation in vertical velocity from the mean air flow, positive upwards

It is to be noted, in passing, that even though the ' mean ' velocity has no persistence in the atmosphere a magnitude and direction for u_h can be calculated over an individual period so that $\overline{\rho u'}$, $\overline{\rho v'}$, $\overline{\rho w'}$, are all zero for that period. The notation is here used in that sense with the further assumption that the mean velocity is horizontal.

Expanding the above expression, we get

$$F = \tfrac{1}{2}u_h^2\,\overline{\rho w'} + u_h\,\overline{\rho u'\,w'} + \tfrac{1}{2}\,\overline{\rho u'^2\,w'} + \tfrac{1}{2}\,\overline{\rho v'^2\,w'} + \tfrac{1}{2}\,\overline{\rho w'^3}$$

Of the terms on the right, the first vanishes because $\overline{\rho w'} = 0$ by definition ; the second is equal to $-\tau u_h$ where τ is the Reynolds's stress at that level and the last three will be grouped together and represented by the symbol δ. In the summarized expression

$$F = -\tau u_h + \delta,$$

the first term represents the flux of mean-flow kinetic energy which is always downward and the second is the rate of diffusion of turbulent kinetic energy across the surface and may act in either direction. When the results are considered, δ will be found negligible in comparison with τu_h at the heights (2 m and 29 m) where measurements were made.

The energy entering the layer at the rate F is derived from the mean flow. To this flux of energy must be added that part of the vertical eddy flux of total heat which is available for transformation into kinetic energy. Priestley and Swinbank (1947) have shown that this flux includes the rate of working against mean pressure forces but their proof is easily extended to include the rate of working against turbulent fluctuations in pressure.

Richardson (1920) has shown that the rate of conversion of total heat into eddying energy per unit volume is equal to $-\dfrac{cg}{c_p}\dfrac{d\sigma}{dz}$

where c is the eddy conductivity in g $\sec^{-1}$ cm^{-1}.
g is the acceleration due to gravity.
c_p is the specific heat of air at constant pressure.
σ is the specific entropy of the air.

This quantity has been evaluated where possible and translated into a rate of conversion per unit mass.

The total rate of dissipation within the layer is given by $\int_0^h \epsilon\rho\,dz$ and it follows that

$$\int_0^h \epsilon\rho\,dz = \tau u_h - \sigma - \int_0^h \frac{cg}{c_p}\frac{d\sigma}{dz}\,dz$$

in the steady state. When the turbulence is assumed to be horizontally uniform and steady in time, Calder's proof (1939) of the constancy of shearing stress with height may be invoked and it follows that

$$\epsilon = \frac{\tau}{\rho}\frac{du}{dz} - \frac{cg}{\rho c_p}\frac{d\sigma}{dz}$$

since, as stated above, δ has been found negligible in the cases considered. Of the two terms on the right, the first is inversely proportional to z when a logarithmic wind profile is assumed and the variation of density with height is neglected and, moreover, will vary approximately as the cube of the wind speed. The second term reduces to the form $-(g/\rho c_p T)H$ where H is the vertical flux of total heat and T is the absolute temperature. This form for the second term, which was derived in another way by Calder (1949) indicates its comparative independence of height since, as Sutton (1948) has shown, the variation of H with height is small in certain conditions. Moreover, since variations in wind speed will have relatively little effect on the amount of heat to be removed from the ground and hence on H, it is to be expected that the second term will vary with wind speed much less rapidly than the first. In the present work, cases have been chosen in which the contribution from the mean flow outweighs that from the total heat flux as shown in Table I and subsequent discussion must be taken as limited to such cases which, as is obvious, will in general be those of comparatively strong wind or of small temperature gradient or those where the height considered is small.

3. Results

While it is not possible to measure directly the local rate of working, $\frac{\tau}{\rho}\frac{du}{dz}$, against the Reynolds's stress in the atmosphere, it is possible to evaluate the flux of mean-flow kinetic energy into a shallow layer and the diffusion of turbulent energy across its upper boundary. This has been done by using certain observations made with hot-wire anemometers by W. C. Swinbank (1951) (runs 1, 2, 3, 4) for the measurement of shearing stress, heat flux and water-vapour flux and by E. L. Deacon (unpublished) with a pressure-plate anemometer and vane for the evaluation of shearing stress (run 5). All observations were made over the same open site near Melbourne with an excellent exposure in all directions. The apparatus used for the observations responds to eddies of period varying from the period of observations (5 min) down to about 2 sec in the case of runs 1-4 and to about 0·1 sec in the case of run 5. However, as will be shown, the observations provide internal evidence that the selection of this range of eddy sizes does not affect the general conclusions to be drawn from the results.

From the shearing stress as calculated by the authors mentioned and the measured wind speeds, values of $u_h \tau$, representing the downward flux of mean-flow kinetic energy, have been obtained. Values of δ, the diffusive flux, have been calculated in the form given above on the assumption of constant density. It is to be noted that, in the case of runs 1 to 4, the shearing stress and diffusive flux were measured at 1·7 m while the mean wind speed was measured at 2·0 m. Since it was considered that τ and δ would change with height more slowly than would u, the vertical flux of energy has been considered applicable to 2·0 m for the purpose of calculating the average dissipation per unit mass over the layer. These results are presented in Table I, together with values of the energy density, $\frac{1}{2}(\overline{u'^2} + \overline{v'^2} + \overline{w'^2})$ and the rate of conversion

TABLE I

Run No.	Ht. of observation h (m)	Time (GMT) and date of observation		Mean wind speed at h (cm sec^{-1})	Shearing stress τ (dyne cm^{-2})	$u_h \tau^*$ (erg cm^{-2} sec^{-1})	δ^* (erg cm^{-2} sec^{-1})	Rate of dissipation averaged over layer 0 to h (erg g^{-1} sec^{-1})	Energy density at h (erg g^{-1})	Temperature difference 2 m to 0·5 m (°F)	$\frac{g}{\rho c_p}\frac{d\sigma}{dz}$ averaged over layer 0·5 m to h (erg g^{-1} sec^{-1})
1	2·0	1145	5.3.51	725	1·94	− 1410	+ 19	5900	3400	− 0·7	− 24
2	2·0	1202	5.3.51	685	1·18	− 810	− 12	3400	5500	− 1·0	− 17
3	2·0	2037	7.3.51	178	0·25	− 45	− 0·4	190	510	+ 0·8	+ 3·7
4	2·0	2207	14.3.51	110	0·38	− 4·2	− 0·7	18	280	+ 3·0	+ 1·3
5	29	1443	27.4.51	741	2·30	− 1700	+ 110**	490	8500**	− 0·7	

* Positive sign indicates an upward flux.

** In this run, measurements of v' were not made. It has been assumed that $\overline{v'^2} = \frac{1}{2}(\overline{u'^2} + \overline{w'^2})$ and $\overline{v'^2 w'} = \frac{1}{2}(\overline{u'^2 w'} + \overline{w'^3})$, since in neither case did the two terms on the right differ greatly from each other.

of total heat into eddying energy according to Richardson's expression, evaluated from the mean temperature gradient over the height interval from 0·5 m to 2·0 m and the values of eddy conductivity calculated by Swinbank (1951).

The calculated values of ϵ do indeed indicate the rate of dissipation averaged over the layer, but it is obvious that most of this dissipation must occur in the very lowest layers where the wind gradients are most intense and it is of greater meteorological interest to estimate the actual rate of dissipation at certain definite levels. All the observations here used were accompanied by a measurement of mean wind speed at 50 cm and thus it is possible, by assuming that δ is still negligible at 50 cm, to evaluate the kinetic energy flowing out of the bottom of the layer 50 cm to h and thus to estimate the total energy dissipated within that layer since τ is constant with height under the assumed conditions. This total must be equal to $\int_{50}^{h} \epsilon\rho \, dz$. As discussed above, it may be taken that ϵ is proportional to du/dz and so to $1/z$ and it is possible therefore to use the calculated total dissipation within the layer to calculate ϵ at particular heights within the layer and even to extrapolate beyond its boundaries. This has been done and the results are presented in Table II. The most striking feature of these results is the very large values obtained for ϵ near the ground, greatly exceeding the average figure quoted by Brunt, as is only to be expected since the latter is an average rate over the whole depth of atmosphere and at higher levels ϵ may well be very small indeed.

When extrapolating upwards, care must be taken not to go as far as heights where the contribution to the dissipation from the vertical flux of total heat becomes important. Comparison of Table II with the last column of Table I shows that this can happen at surprisingly small heights. However, in order to make clear the relative magnitudes of the two contributing terms, values of ϵ as given in Table II have been left as they were calculated on the assumption that only the contribution from the mean-flow kinetic energy is important.

TABLE II

Run No.	Ht. of observation $h(m)$	Wind speed at h (cm sec^{-1})	Wind speed at 50 cm (cm sec^{-1})	Energy dissipated within layer 50cm to h (erg cm^{-3} sec^{-1})	ϵ (erg g^{-1} sec^{-1}) at 1 m	2 m	5 m	10 m	20 m	30 m	50 m
1	2·0	725	528	385	2310	1150	460 *	230 *	115 *	77 *	46 *
2	2·0	685	500	120	720	360	140 *	72 *	36 *	24 *	14 *
3	2·0	178	115	16	96	48	19 *	9·6*	4·8*	3·2*	1·9*
4	2·0	110	30	2·8	17	8·4	3·4*	1·7*	0·8*	0·6*	0·3*
5	29	741	327	950	1950	975	390	195	98	65 *	39 *
		mean of day results			1660	830	330	170	83	55	33
		mean of evening results			56	28	11	5·6	2·8	1·9	1·1

* extrapolated

On the occasion of 5 March 1951 when 2 runs were made with an interval of 17 min between them it is possible to look rather more fully at the energy balance of the 50 to 200 cm layer. The observed sources of energy for the layer are as follow :

(a) Flux of mean-flow kinetic energy into the layer (average of runs 1 and 2), 1,400 erg g^{-1} sec^{-1}.

(b) Conversion of heat into eddying energy (average of runs 1 and 2), 20 erg g^{-1} sec^{-1}.

The observed accumulations of energy in the layer are :

(a) Increase in total heat. Over the seventeen-minute period, an increase of temperature (average of 2 m and 0·5 m readings) of 0·9°F was observed. This represents an accumulation of energy at the rate of 5,100 erg g^{-1} sec^{-1}.

(b) Increase in energy density. Over the period an increase of 1,900 erg g^{-1} was observed at 2 m. This represents accumulation of energy at the rate of 2 erg g^{-1} sec^{-1}.

(c) Decrease of mean-flow kinetic energy. During 17 min, the change in wind speeds at 2 m and 0·5 m as shown in Table II corresponds to a loss of kinetic energy at the rate of 21 erg g^{-1} sec^{-1}.

Two points of interest emerge here. In the first place is the comparative unimportance of the terms representing the rate of conversion of heat into eddying energy as stressed above and the rate of change of turbulent and mean-flow kinetic energy. Secondly we see that the dissipation of kinetic energy within the layer, which is usually thought to be of minor significance, accounts for over one quarter of the observed rate of rise of temperature. The remaining part of the rate of temperature rise can be accounted for in two ways. First, an interval of 17 min represents a wind travel of over 3 mi at the actual speeds obtaining. Air must therefore be brought from well beyond the boundaries of the uniform site and the possibility of advective effects over such a period cannot be excluded. With these effects the present treatment

is not equipped to deal. Secondly, and probably more important, we have hitherto discussed only that part of the total heat flux which is converted to kinetic energy according to Richardson's expression. In fact, however, it is very likely that a greater part of the flux than this remains in the layer and contributes directly to its warming. It is not possible to discuss this point further until accurate observations of the variation of heat flux with height are available. Furthermore, no account has been taken of direct heating of the air by radiation. Such considerations of energy balance serve to underline the importance of the rate of dissipation of kinetic energy, as far as the lowest layers of air are concerned.

There is good agreement between runs 1 and 5 for the vertical flux of mean-flow kinetic energy which, considering the difference between the characteristics of the instruments used and the similarity between the two mean wind speeds, indicates that eddies of period between 0·1 sec and 2 sec do not make a large contribution to this flux at the levels considered. The agreement between diffusive fluxes is not as good but both remain negligibly small. Again, the small rates of change of mean flow and turbulent kinetic energy of the layer shown by the two runs on 5 March 1951 can be associated with eddies too long in period to be detected directly and indicate that these eddies are ineffective as transporting agents.

4. Decay of turbulence

An interesting time parameter of the flow, t_0, is formed by taking the ratio of the energy density to the rate of dissipation. This represents the time which would be required for the existing turbulent energy to decrease to zero if it were not being constantly replaced from the mean-flow kinetic energy and if its decay were linear in time. The time parameter, t_0, can be converted into a characteristic-length parameter, x_0, by multiplication by the mean wind speed existing at the time. These quantities, t_0 and x_0, have been calculated for run 5 at 29 m using the observed energy density of 8,500 erg g^{-1} and an interpolated ϵ of 67 erg g^{-1} and their values are, respectively 127 sec and 940 m. Now it is extremely interesting to note that the latter figure is about the distance required to set up a wind profile, characteristic of the terrain, to the height of observation. E. L. Deacon (unpublished) has estimated a distance of 850 yd of travel as being necessary to establish a characteristic profile to a height of 30 m over grassland of similar nature using, as basis, data published by Schlichting (1936) on the distance required to set up a boundary layer of given thickness. The establishment of a wind profile must be intimately connected with the processes of energy decay but the exact mechanism must, for the present, remain obscure.

5. Eddy sizes

It is of interest to compare the ratio of ϵ to the turbulent-energy density E with that derived by Richardson (1920) by assuming a particular model of eddy structure and assigning an arbitrary dimension of 10 m to it. In this way he obtained a value of $2{\cdot}6 \times 10^{-6}$ sec^{-1} for ϵ/E with ϵ/E inversely proportional to the square of l the dimension of the dissipating eddies. From his figure one can estimate the size of eddy which would, in the same model, account for the ratio of interpolated ϵ to observed E at the height of observation. Values have also been calculated for G. I. Taylor's microscale of the turbulence, λ, (Taylor 1935) using the relationship

$$\epsilon = 10\,\nu\,\frac{E}{\lambda^2}$$

where ν is the kinematic viscosity of the air. This relationship is strictly valid only in the case of isotropic turbulence but the results are of sufficient interest to justify the approximate evaluation.

The two estimates, l and λ, of the characteristic length of the smallest eddies with which the greatest dissipation is associated are presented in Table III. The good agreement between l and λ will be noted as also will the variation with height in the expected sense.

TABLE III

Run No.	l (cm)	λ (cm)
1	2·8	2·1
2	6·3	4·7
3	5·3	3·9
4	9·3	6·9
5	18·0	13·0

6. Acknowledgments

The author expresses his thanks to Mr. W. C. Swinbank and Mr. E. L. Deacon not only for permitting the use of their observations but also for much helpful discussion and advice.

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551.506.7(261)(4)
551.557.35 : 551.547

Structure of the upper westerlies ; a study of the wind field in the eastern Atlantic and western Europe in September 1950

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(Manuscript received 8 January 1952)

SUMMARY

On the basis of 60 cross-sections over western Europe and the northeast Atlantic, two for each day of September 1950, it is concluded that:

(a) The westerlies always contain one or more wind maxima in the upper troposphere with structure broadly similar to that described by other writers on jet streams but with much variety of intensity and velocity profile.

(b) The mean horizontal wind shear on the warm side of the jet stream is about two-thirds of that on the cold side where it averages 20 kt/100 mi and may exceed 100 kt/100 mi.

(c) The geostrophic relation gives a broadly correct representation of the true winds.

(d) There is a broad connexion between fronts and jet streams, but the relationship is often less simple than that suggested in published work hitherto. Divided jets, distinct double jets and other complexities arise.

Daily charts are given showing the jet axes and intensities at 300 mb superimposed on the surface analysis.

1. INTRODUCTION

The notes presented here are mainly the outcome of an examination of 60 cross-section charts depicting the wind and temperature fields through the extratropical westerlies during September 1950.

The positions of the sections are shown in Fig. 1. One section extends from near Angmagssalik across the British Isles to near Malta (for brevity, called section AM); the other is from near Lagens across southern England to southeast of Stockholm (called section LS). The upper-air stations used in the construction of the sections are also shown in Fig. 1. Some of these observations are appreciably off the line of the section. Consideration of the synoptic situation usually enabled a decision to be made as to the representativeness or otherwise of these ' off-the-line ' observations. Thus it was necessary on some occasions to neglect and on other occasions to modify the observations reported (this applied to the temperature field). The wind field, represented by the components of the wind perpendicular to the line of the section (positive for flow from the Atlantic through the sections to Europe – negative for the reverse direction), was generally based on wind observations, but occasional measurements of geostrophic wind were made, particularly in the regions Angmagssalik to the Hebrides and Lagens to Cornwall, when actual wind reports were either missing or non-representative. The sections were drawn daily on the basis of the 1400 GMT upper-air observations.

Since the publication of the paper ' On the general circulation of the atmosphere in middle latitudes ' (Staff Members, University of Chicago 1947), a considerable number of cross-sections have been constructed through selected jet streams. However, the fixed sections AM and LS were chosen in order to study the general wind and temperature fields, although with emphasis on the jet-stream phenomenon. Our work with the September 1950 fixed sections has suggested several aspects of the jet stream requiring detailed and special investigation.

Figure 1. The lines of vertical section. Abbreviations are LA = Larkhill, LI = Liverpool, D = Downham Market and C = Copenhagen.

2. Broad features

Figures 2 and 3 are examples of wind fields which most commonly occurred. One third to a half of the section is dominated by a strong current having a maximum of velocity normal to the section of 80 kt or more. The rest of the field is weak and featureless by comparison, although it may contain local maxima of the order of 20 kt to 40 kt at any level of the troposphere or, infrequently, in the stratophere. On all but five of the sections there could be found currents with maximum (normal) speed greater than 60 kt; on these five sections there were local maxima of 45 kt to 60 kt and the complementary sections for the same day showed maxima of 75 kt at least. Thus the series of cross-sections clearly demonstrated the existence, every day of the month, of wind maxima of the jet-stream type in the upper troposphere.

In general terms the sections show that every tropospheric air current on the synoptic scale has a maximum, located somewhere in the central part and near or just under the tropopause. More than one maximum sometimes occurs in the upper troposphere; this seems usually to be associated with the splitting of a main current into two subsidiary branches in a region of confluence or difluence. Distinct maxima often occur in the two branches separated by distances of 500 mi or more but may be continuous with more closely spaced maxima within the core of the mainstream.

The number and location of wind maxima above 500 mb along each section during the month are displayed graphically in Figs. 4 and 5. These values represent the wind-speed maxima and not merely the maxima of the components normal to

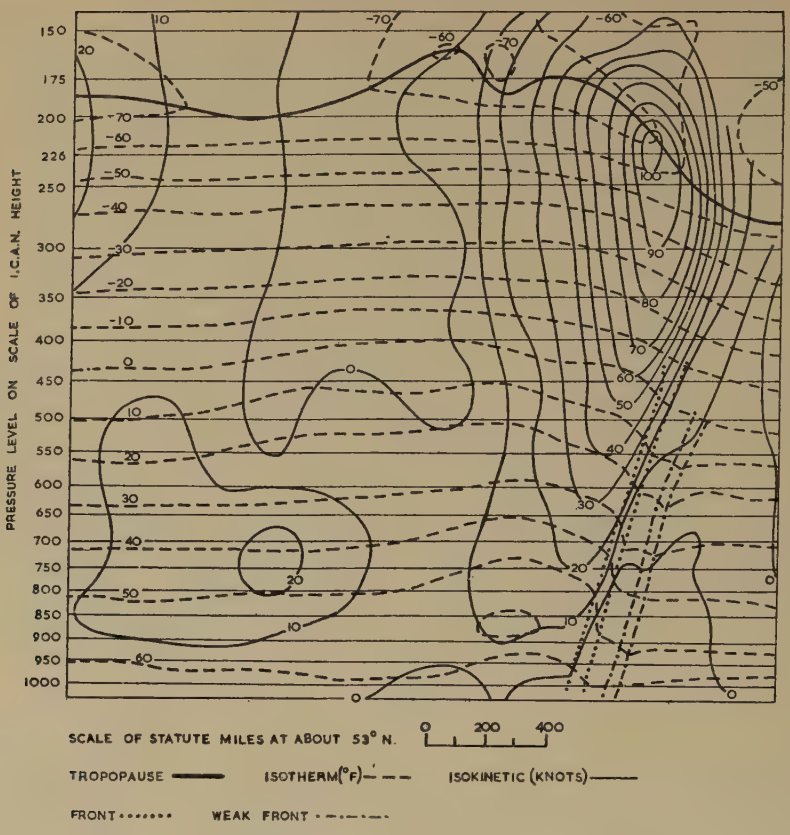


Figure 2. Cross-section Lagens – Stockholm 1400 GMT 4 September 1950.

the sections. The wind maxima tend to be located at a somewhat higher level in lower than in higher latitudes (see Fig. 4); in the neighbourhood of the British Isles at least, the wind maxima are most commonly between 300 and 200 mb with rather few maxima outside these pressure levels; currents flowing from Europe through the sections to the Atlantic (negative figures) are uncommon. These results are not unexpected.

Further information concerning the frequency of occurrence of wind maxima within different pressure intervals during September 1950 is presented in Figs. 6 and 7 – these figures also contain the mean value of the wind maxima in the pressure interval. In round figures, 70 per cent and 40 per cent of the wind maxima were greater than 80 kt and 100 kt respectively.

3. Typical jet-stream structure

The jet stream associated with a frontal zone has been described by Palmén (1948), by Durst and Davis (1949) and by other writers. Our cross-sections contain jet streams with similar structure (e.g., Fig. 2), although with appreciable difference in detail between individual wind maxima.

The high wind velocity is evidently accounted for in the main by the integration throughout the troposphere of the thermal-wind effect. In most cases the subtraction

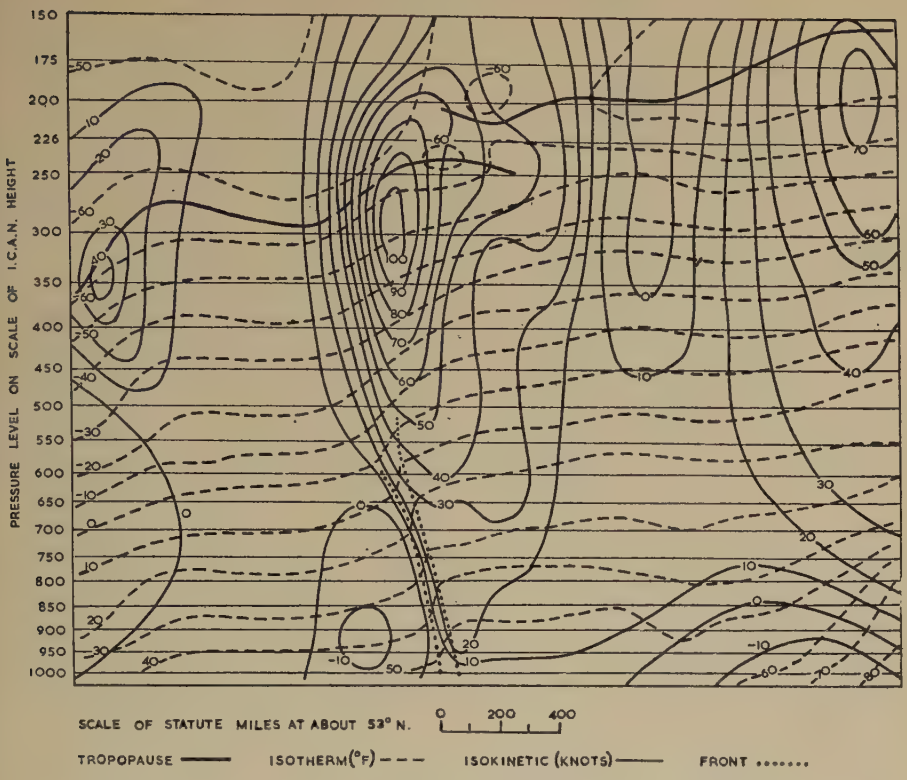


Figure 3. Cross-section Angmagssalik – Malta 1400 GMT 30 September 1950.

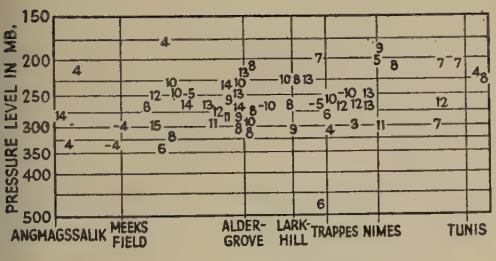


Figure 4. Section Angmagssalik-Malta.

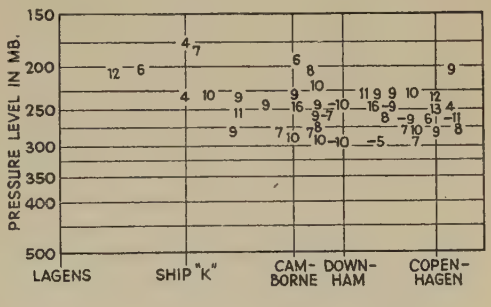


Figure 6. Section Lagens-Stockholm.

Figures 4 and 5. Positions and magnitudes of centres of maximum velocity in airstreams crossing the sections Angmagssalik-Malta and Lagens-Stockholm daily at 1400 GMT in September 1950. The figures give the velocity at the centres of the maxima in tens of knots (positive for flow from the Atlantic through the section to Europe).

of the surface-velocity field would have little effect on the general form and magnitude of the isokinetics of the jet stream. In the lower troposphere strong horizontal thermal gradients are usually contained within narrow sloping frontal zones and are found in conjunction with a vertical wind shear. However, the thermal-wind field associated with the simple model of a sloping boundary layer is not sufficient to produce large wind maxima of the type observed. In fact the jet streams are located within zones

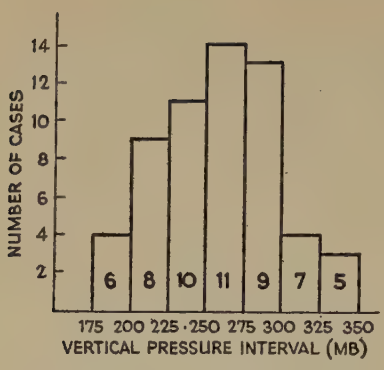


Figure 6. Section Angmagssalik – Malta.

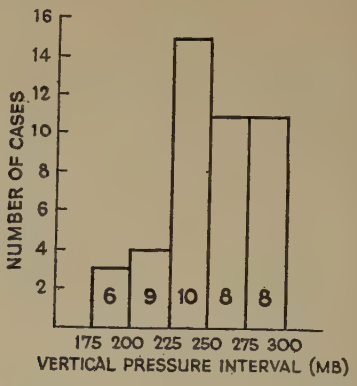


Figure 7. Section Lagens – Stockholm.

Figures 6 and 7. Frequency diagrams of the pressure level of the wind maxima of September 1950. Figures inside the histograms give the mean value of the wind maxima (tens of knots) occurring in the given pressure interval.

of the upper troposphere in which the isotherms slope relatively steeply and with marked horizontal thermal gradients extending throughout a deep layer in the vertical. It seems that although two air masses are normally separated in the lower troposphere by a frontal zone (which may be very ill-defined at times), the thermal contrast aloft is usually spread over a much wider vertical band. When there is a well-defined sloping frontal layer near the surface it is often difficult to detect above 500 mb and most commonly merges into the wider vertical column aloft which shows up as a broad inflexion in the isotherms (i.e., strong baroclinity). The tropopause is instrumental in introducing reversed thermal gradients, and a decrease in wind speed above the jet stream. The sharp diminution in wind speed towards the colder (tropospheric) side at the level of the wind maxima is generally closely associated with a very steep tropopause (or possibly with a discontinuity in the tropopause); both factors, almost of necessity, seem to occur together.

4. Wind shears associated with jet streams

(*a*) *Horizontal wind shears*

The kind of shear that occurs may be illustrated from profiles of the wind speed across the jet streams, based on the September 1950 sections.

Fig. 8 contains three mean wind-speed profiles constructed from cold-front-type jets (i.e., jets which tend to move laterally towards the warmer tropospheric air) hereafter called type c jets, from warm-front type jets (i.e., jets which tend to move laterally towards the colder tropospheric air) hereafter called type w jets, and from jets of both types. Not all the jets that showed up on the 60 fixed sections were utilized in the construction of Fig. 8, but only those well substantiated by actual wind reports. In addition only those jets were considered whose direction deviated by less than 45° (in most cases it was less than 25°) from the line normal to the section; this restriction ensured that the major characteristics of the jet stream were given by those of the jet component perpendicular to the section. Altogether 23 separate wind profiles were constructed, although only 21 cases were employed in computing Fig. 8, as two profiles did not extend sufficiently far on each side of the wind maxima.

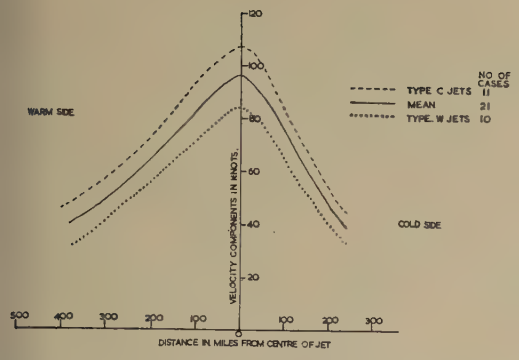


Figure 8. Mean velocity profiles at the levels of the centres of jet streams.

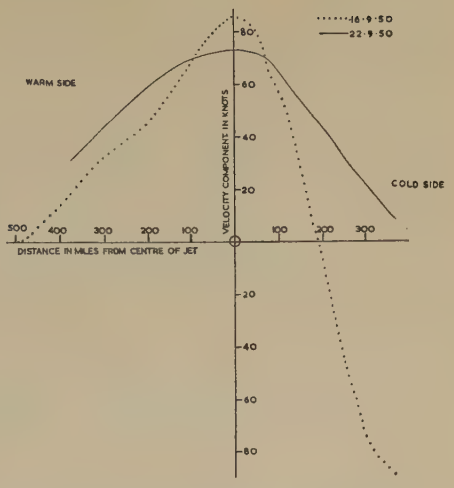


Figure 9. Velocity profiles at the levels of the centres of the jet streams of 16 and 22 September 1950 (section LS).

From Fig. 8 we see that the three profiles are broadly similar. In each profile the horizontal wind shear is greater on the cold side than on the warm side of the jet axis. The intensity of the mean type c jet is some 20 kt greater than that of the mean type w jet, yet the horizontal shears are about the same within the main body of the current. On the cold side the wind decreases by about 18 kt in the first 100 mi, and equals about half the maximum speed some 200 mi from the jet axis; whilst the wind diminution is about 14 kt in the first 100 mi on the warm side, with the wind falling off to half the maximum value about 320 mi from the jet axis. In the average the wind shear on the warm side is roughly two-thirds of the shear on the cold side.

Fig. 8 depicts average conditions. There is considerable variation in the shape of the wind maximum and the value of the wind shears from jet to jet. Two interesting cases are presented in Fig. 9 (section LS, 16 and 22 September 1950). The profile of 16 September has a sharp maximum with a large wind shear of the order 60 kt to 70 kt in 100 mi over at least 200 mi. This is an example of two contrary jets quite near each other. The wind shear on the cold side, centred about the zero speed, averaged 100 kt over 100 mi at the 300 mb level. A different type is shown as the profile of 22 September; here the wind maximum is quite flat and the wind shears small in comparison with those of 16 September.

It has already been mentioned that the wind profiles are based on wind components normal to the sections. If we had varied the position of the sections in order to have them more nearly perpendicular to the individual jets slightly greater maxima and shears would be recorded. Our estimate is that the maxima in Fig. 8 would be increased by 10 kt or so, with the mean shears perhaps 10 per cent greater than given.

(b) *Vertical wind shears*

The vertical wind shears are rather variable from jet to jet. The greatest shears normally occur in the troposphere below and on the cold side of the jet. If the frontal

zone is marked, as in Fig. 2, there is usually an associated concentration of shear with values of the order 10 kt to 15 kt per 1000 ft. Sometimes, however, the thermal contrast is spread over a wide zone and then the shear may be rather uniform in the troposphere, perhaps not exceeding 5 kt per 1000 ft anywhere. On the warm side of the jet the shears tend to be rather uniform throughout the troposphere, being often only a few knots per 1000 ft. The vertical shears above the jet in the stratosphere vary considerably from case to case. As has been suggested by Durst and Davis (1949) the total change of wind is often spread more evenly over a deep stratospheric layer (of the order 12,000 ft) than in the troposphere. But this is not always the case. Quite often a very strong horizontal thermal gradient opposed to that in the troposphere sets in immediately above the tropopause, associated with vertical shears of about 10 kt to 15 kt per 1000 ft, as indicated in Fig. 2. On occasion the shear just above the tropopause may even be two or three times greater than the shear anywhere in the troposphere (e.g., case of jet near Aldergrove on 24 September 1950).

5. Jet streams in relation to 300 mb charts

Experience with the cross-sections shows that the general structure of the wind is governed by the geostrophic relation. The wind maximum at 300 mb is normally located just under the axis of the jet stream, while the wind profile at 300 mb is broadly similar to that at the level of jet maximum. If, therefore, the 300 mb contour charts are correctly drawn they should give a good indication of the location and intensity of the jet stream. In routine work, however, the 300 mb contour charts must be

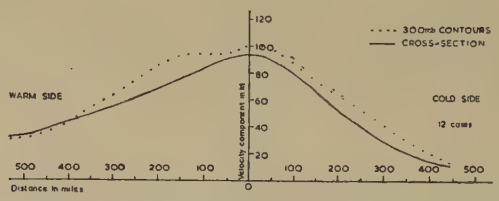


Figure 10. Type c.

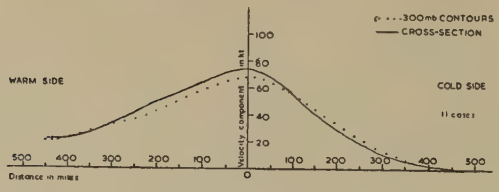


Figure 11. Type w.

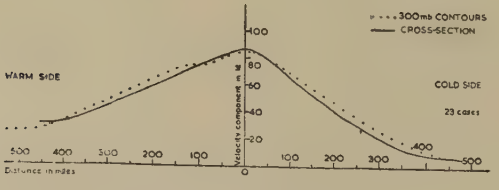


Figure 12. Overall mean.

Figures 10-12. Mean 300 mb wind profiles for selected wind maxima of September 1950

constructed rapidly, there is rarely time to make a detailed cross-sectional study and the contours may not give a very close representation of the true structure.

A comparison was made between the wind profile at 300 mb as given by (a) the cross-sections (the cross-section wind) and (b) the routine 300 mb contour charts prepared at Central Forecasting Office, Dunstable, (the 300 mb contour wind). On each of the occasions of jet stream given by the cross-sections used in Section 4 (see Fig. 8) the 300 mb contour wind component normal to the appropriate section was measured at suitable points (by measuring the mean geostrophic wind over 400 ft contour intervals which overlapped by 200 ft). Individual and mean 300 mb contour wind profiles, as well as individual and mean cross-section wind profiles at 300 mb, were constructed.

The mean profiles of cross-section and 300 mb contour wind for the type c jets, the type w jets and both types together are shown in Figs. 10, 11 and 12 respectively. It will be noted that, in spite of the small number of cases, the mean cross-section and 300 mb contour wind profiles are remarkably similar, indicating that, in the mean, the supposition that the wind maximum obtained by geostrophic measurement of 300 mb contours should be a good representation of the position and intensity of the jet-stream axis is justified. However, examination of the individual profiles gives a rather less satisfactory picture.

Occasionally a 300 mb contour wind profile (based on working charts) exhibits two maxima as in Fig. 13. More often than not these maxima merely arise as an accident in the drawing of the chart. Smoothing the profile will generally produce something more comparable with reality, although on a few occasions two wind maxima do in fact exist, so that the smoothing process may seriously increase the discrepancy between the 300 mb contour and cross-section wind profiles.

It is believed that the main reasons for the differences between the 300 mb contour and cross-section wind maxima are :
(1) contour-height errors, (2) unintentional introduction of unreal gradients, (3) the geostrophic approximation.

We consider that (1) contributes the main error. An examination of some 300 mb charts has clearly shown that unreal gradients are often introduced by too slavish acceptance of height observations as correct (marked fluctuation in contour heights over the period 6 hr before until 6 hr after the time of the chart often indicates that the contour heights are in error). The errors introduced by (2) are less important, but could be reduced if sufficient time were spent on the preparation of the charts. There is no reason to attempt to improve on the geostrophic approximation so long as errors of the type (1) and (2) remain.

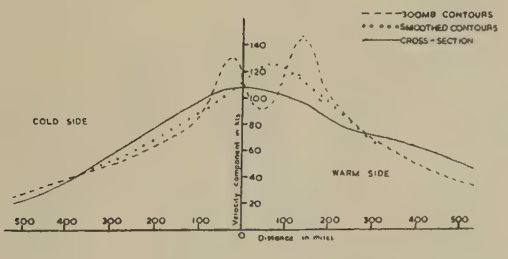


Figure 13. 300 mb velocity profile 29 September 1950 (type c)

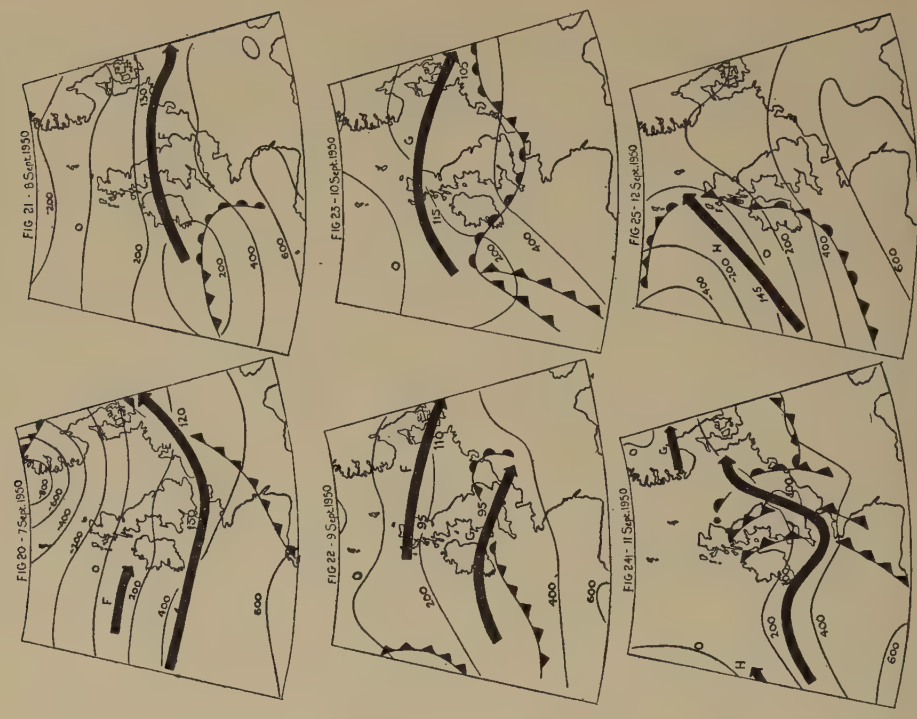


FIG 21 - 8 Sept. 1950
FIG 23 - 10 Sept. 1950
FIG 25 - 12 Sept. 1950
FIG 20 - 7 Sept. 1950
FIG 22 - 9 Sept. 1950
FIG 24 - 11 Sept. 1950

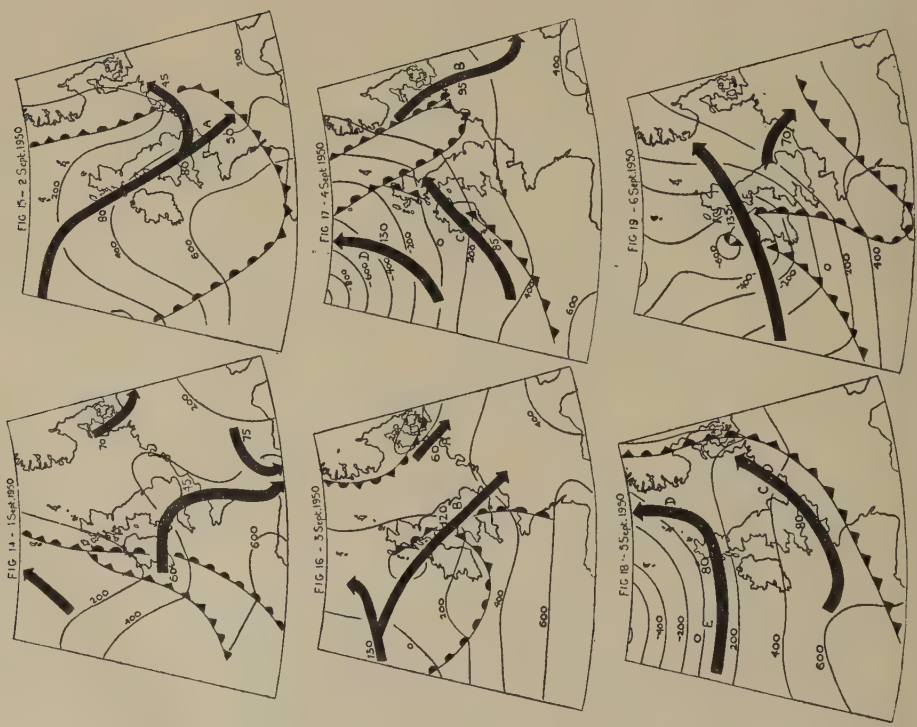


FIG 15 - 2 Sept. 1950
FIG 17 - 4 Sept. 1950
FIG 19 - 6 Sept. 1950
FIG 14 - 1 Sept. 1950
FIG 16 - 3 Sept. 1950
FIG 18 - 5 Sept. 1950

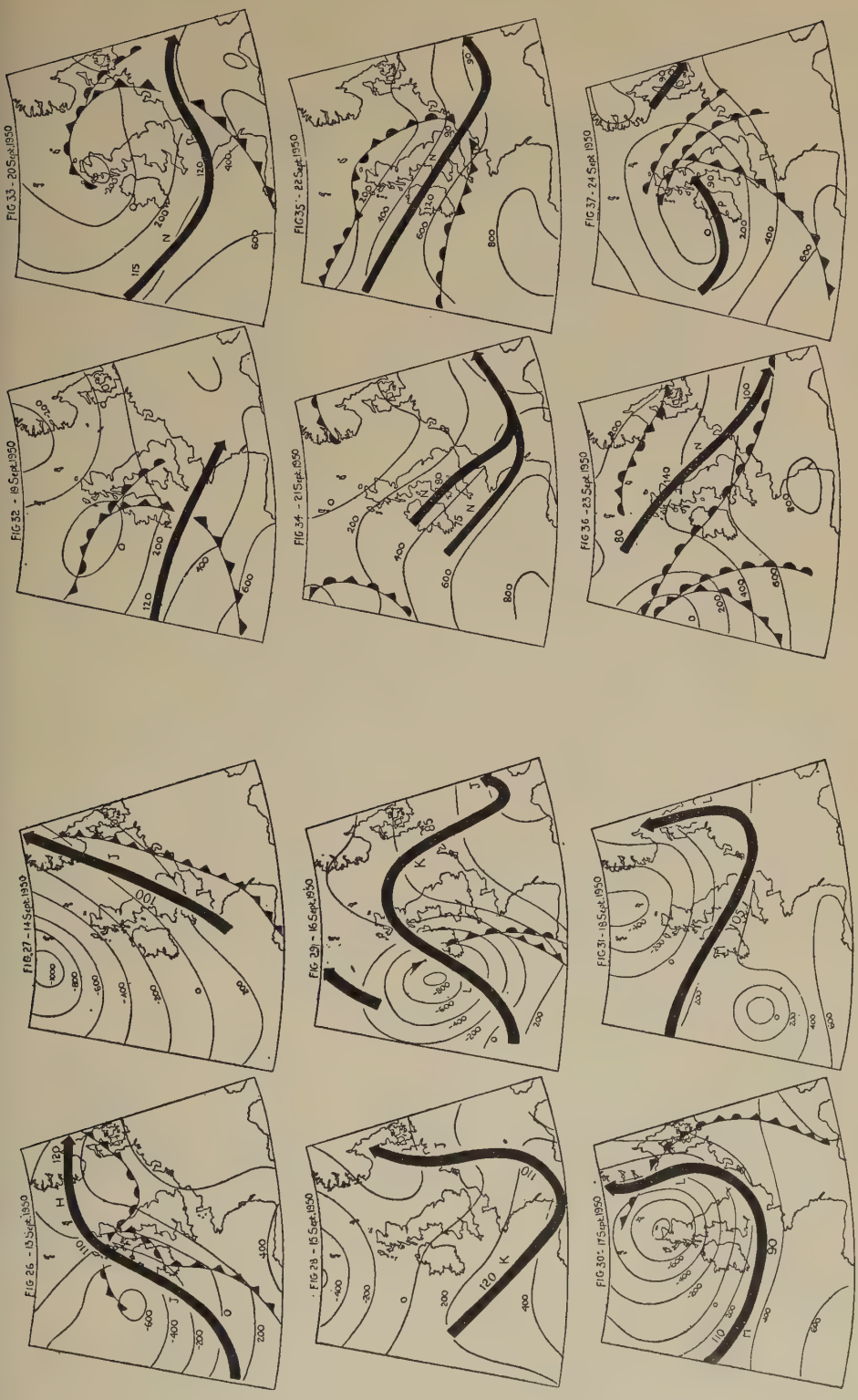
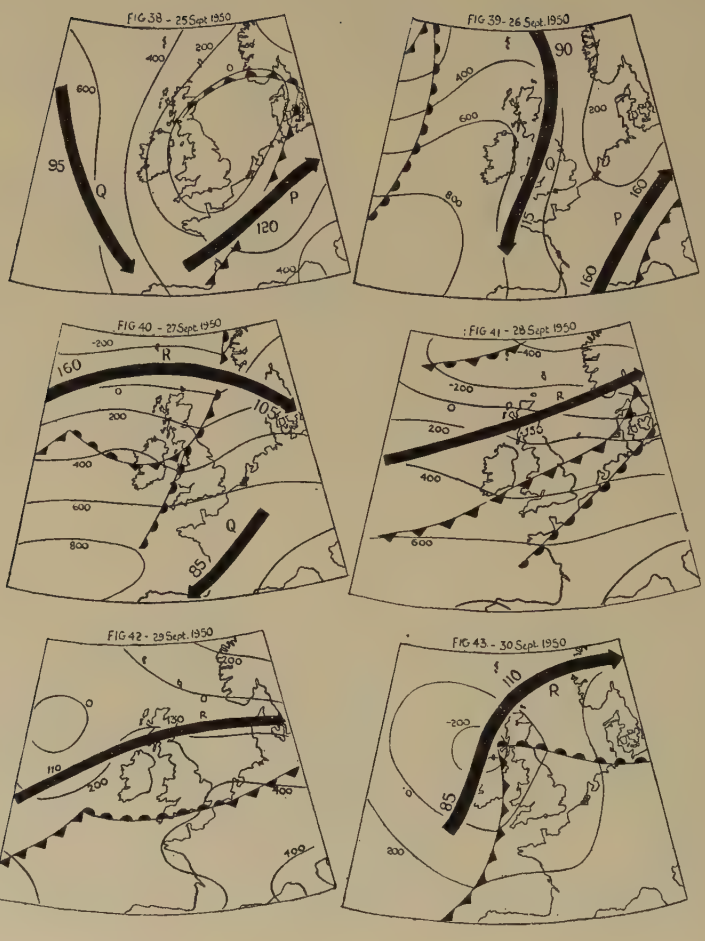


FIG 25 - 13 Sept. 1950
FIG 26 - 13 Sept. 1950
FIG 27 - 14 Sept. 1950
FIG 28 - 15 Sept. 1950
FIG 29 - 16 Sept. 1950
FIG 30 - 17 Sept. 1950
FIG 31 - 18 Sept. 1950
FIG 32 - 19 Sept. 1950
FIG 33 - 20 Sept. 1950
FIG 34 - 21 Sept. 1950
FIG 35 - 22 Sept. 1950
FIG 36 - 23 Sept. 1950
FIG 37 - 24 Sept. 1950



Figures 14 to 43. Association between jets and surface fronts at 1400 GMT each day during September 1950. Fronts indicated in conventional manner. Jet as broad arrow with 300 mb speed in knots at selected points. 1,000 mb contours (ft) as thin lines. Individual jets identified from chart to chart by letters A, B etc.

The conclusion reached is that the 300 mb contours as drawn under working conditions give a rough indication of the position and intensity of the jet-stream axis. Experience with upper-air charts and cross-sections suggests, however, that the additional aids arising from continuity in the charts and maximum utilization of observed winds enable reasonably accurate positioning of the jet-stream axis to be made. As an aid to analysis and forecasting of the 300 mb wind field it is useful to mark the estimated positions of jet-stream axes on 300 mb charts in much the same way as fronts are marked on surface charts. This simple procedure materially helps in maintaining continuity from chart to chart and in preventing the introduction of unreal gradients.

6. Association between jets and surface fronts

When warm and cold air masses are brought closely together there are created strong horizontal temperature gradients with which are normally associated fronts and jet streams. Generally speaking, a well-marked (in a thermal sense) surface front is sufficient but not necessary evidence for the existence of a jet stream aloft. Quite strong wind maxima may exist in association with weak surface fronts. This arises by virtue of the fact that strong currents aloft can be produced by broad thermal contrasts over a sufficiently deep layer, whereas the thermal contrast is normally concentrated in a rather narrow band at a well-marked front. The position of the jet-stream axis in relation to frontal surfaces is generally in the uppermost part of the warm troposphere approximately above the intersection of the front with the 500 mb surface, as has been pointed out by several writers (e.g., Palmén 1948). This empirical rule is a fairly good approximation to reality in cases of strong jets and fronts. The rule implies that the jet-stream axis should often be 200 mi to 400 mi behind a surface cold front and 400 mi to 800 mi in advance of a surface warm front. In many cases much greater complexity exists in the relationship between surface fronts and jet streams, as can readily be observed from Figures 14 to 43, which give the estimated positions of the jet-stream axes and surface fronts daily at 1400 GMT in September 1950 in the neighbourhood of the British Isles. The positions of the jet streams were obtained from consideration of the series of cross-sections and the 300 mb charts (available at 6-hourly intervals). Afterwards the surface fronts and 1000 mb contours (copied from the working charts prepared at the Central Forecasting Office, Dunstable) and the jet streams were put on the same charts. It should be mentioned that the thick lines representing the axes of jet streams have generally been terminated where the wind maxima become ill-defined, although in some cases west of the British Isles no stream is indicated simply because of uncertainty in locating it in that region. An indication of the variation of wind speed from one jet to another and at different positions on the same jet is also given in Figures 14 to 43 (the estimates of speed referring to the 300 mb level). Individual jets may be identified from chart to chart by attached letters A, B etc.

Many examples (e.g., Figs. 26 and 39) show jet streams in advance of warm fronts and in the rear of cold fronts in approximately the position that one might expect. In most cases the jets are parallel to the surface fronts only over a limited distance; generally the jets tend to approach that part of the front which is near to the cyclonic centre and to recede from the other end of the front, as in Fig. 26.

Small running waves on a slow-moving cold front affect the associated jet hardly at all. If the wave develops, the jet becomes distorted in sympathy with the increasing amplitude of the wave-like surface disturbance. As the occluding process proceeds the jet tends to weaken in the neighbourhood of the surface centre, perhaps lying almost perpendicular to the surface occlusion, as in Fig. 40. On other occasions the original jet separates out into two distinct jets – a type C and a type W jet – as in Fig. 37. Warm and cold occlusions, having the character of warm and cold fronts, may sometimes be associated with type W and type C jets respectively.

These remarks are in the nature of broad generalizations. On occasion there appears to be little or no association between surface fronts and jets, as in Fig. 16. This state of affairs is likely to arise when the surface fronts are thermally weak, or when a new strong jet is overtaking an older, degenerating frontal system, as in Fig. 16. The remnants of degenerating jets may also complicate the picture. These complexities are inevitable in an atmosphere essentially dynamic in its behaviour. However, over a short period, say of the order of 12 hr, there is considerable conservatism in the relationship of the jet with its front; and this fact can be of some use in short-range forecasting. For instance, different type W jets may be differently aligned in relation to warm fronts, but a particular relationship, once established, is likely to persist for a short period at least.

7. Some points requiring investigation

As has been stated in Section 1, this paper purports only to describe and discuss certain broad features of the wind field in the westerlies that appear to be of significance in synoptic practice. However, there are a number of questions (not all of which were suggested by the work with the September 1950 cross-sections) which deserve brief mention here, although firm answers cannot be given with any degree of confidence at this stage. Further investigations, entailing the construction of many cross-sections at frequent intervals in different positions, are called for.

(*a*) *Movement and distortion of jets*

A common tenet is that a jet propagates itself along its length with only slight lateral motion. It is clear that this is only a tendency at the best. Intense jet streams quite often behave in this manner, but there are great deviations in particular cases, with some jets, particularly smaller ones, often moving quite rapidly normal to themselves. Long and nearly straight jet streams have a certain persistency, but ultimately they become very distorted with associated great changes in the synoptic type, which are fundamentally important in the forecasting problem.

(*b*) *Double jets and splitting jets*

Double jets and splitting jets are indubitably real phenomena, but there are some details about them that are not clear. In particular, the type of wind profile near the region of ' difluence ' in the flow is uncertain. In the case of jets which have branched off from a parent jet the relative intensity (and change in relative intensity) of the two branches (usually one with cylonic and the other with anticyclonic curvature) may be important in synoptic development.

(*c*) *Effect of subsidence*

Markedly different rates of subsidence in neighbouring air columns may set up a horizontal temperature gradient which may increase or diminish the pre-existing temperature gradient and hence modify the jet-stream structure. It is not clear how important is the effect. An example of the part played by subsidence is the tendency for some type c jets to lag behind cold fronts, possibly due to pronounced subsidence in the troposphere just in the rear of the surface cold front.

(*d*) *Tropopause structure*

The structure of the tropopause near jet streams is complex at times. There appear to be three possibilities at least : (1) the tropopause is discontinuous, (2) it is continuous but very steep, (3) it is ' folded.' There is evidence – albeit inconclusive – that all these states can exist, and it may be that they fit into the life-cycle of a jet stream in a manner requiring elucidation.

Acknowledgments

We are grateful to Dr. R. C. Sutcliffe and Mr. J. S. Sawyer for much helpful advice. Acknowledgment is due to the Director, Meteorological Office, for permission to publish this paper.

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The evaporation of raindrops

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(Manuscript received 21 September 1951, in revised form 31 January 1952)

Summary

The evaporation of a single drop of water as it falls through an atmosphere in which the lapse rate is 6·5°C/km and the surface temperature either 15°C or 41°C is examined. By making certain justified assumptions, it is shown that the change in surface area of a large drop (i.e., with radius exceeding 0·15 mm) as it falls from height z_1 to z_4 is proportional to $(1 - f)^{1\cdot13} \exp(-lz)$ where $100f$ is the relative humidity and l is a constant. Values are given for the constant of proportionality and for l. If the drop has a radius less than 0·15 mm the change in volume of the drop is proportional to $(1 - f)^{1\cdot068} \exp(-lz)$.

These results are then used to assess the effect of evaporation on the size and size distribution of raindrops falling through a constant atmosphere. It is shown that if the size distribution of raindrops at some initial height is given by $1 - F = \exp[-(2a/b)^n]$ where F is the fraction of liquid water comprised by drops with radius less than a, and b and n are constants, then evaporation will lead to a change in the values of b and n but will not affect the general validity of the formula. As the rain falls through the non-saturated air the distributive index n tends to a value between 3·5 and 4·0 whether it was greater or less than such a value initially. If the initial value of n is less than 3·0 the scale diameter b increases as a result of evaporation. If the initial value of n exceeds about 3·5 the scale diameter decreases. The effect of evaporation upon the radar response from falling rain is also examined.

In the last part of the paper the effect of the evaporation of rain into air which is initially unsaturated is considered. It is shown that the air temperature tends to a steady value and the relative humidity to 100 per cent. The excess of temperature above the steady-state value and the deficit of relative humidity below 100 per cent decrease exponentially to zero with time. The depression of the steady-state temperature below the initial temperature is a function only of the initial temperature and relative humidity and a table is given showing the value of the steady-state temperature for nine different sets of initial conditions. The rate at which the final state is approached depends however only upon the rate of rainfall and a formula is derived for this dependence.

1. Introduction

The evaporation of a drop of water in an air stream is determined by the rate of transfer of heat and water vapour between the drop surface and the ambient air. If these transfer processes are governed by molecular diffusion the appropriate equations take a well known form. If there is a substantial relative velocity between the drop and the medium, or more correctly if Reynolds's number is not small, the transfer processes are governed by eddy diffusivity. In such circumstances it is customary to insert a velocity coefficient into the transfer equations.

The formulation of the appropriate equations for the evaporation of water drops presents little difficulty. Exact solution of the equations is not so easy. In the following paragraphs awkward expressions in the equations are replaced by expressions having a simpler form in order to permit integration of the equations. The simpler forms are derived by evaluating the inconvenient expressions and fitting simpler formulae to them by empirical methods. The results may lose slightly in accuracy but it then becomes possible to express the effects of evaporation in terms of familiar variables.

When dealing with a collection of drops, i.e., rain, no account has been taken of such factors as downdraughts which are associated with thunderstorm rains.

PART I.—The evaporation of a single drop in a constant atmosphere

2. Heat-balance equation for a falling drop

In a stagnant medium the usual processes of molecular diffusion of heat and vapour lead to the following equations for an evaporating drop of liquid :

$$\frac{dm}{dt} = -\frac{4\pi MDa}{R\phi}(p_T - fp_\theta) \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{dH}{dt} = 4\pi ka(\theta - T) \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where

m = mass of drop
t = time
M = molecular weight of liquid (18 for water)
D = coefficient of diffusion of water vapour in air ($cm^2\ sec^{-1}$)
R = gas constant ($= 8{\cdot}314 \times 10^7$ erg $deg^{-1}\ mol^{-1}$)
p_T, p_θ = saturation vapour pressure at temperature T, θ
fp_θ = vapour pressure in ambient air
θ = temperature of ambient air
T = surface temperature of drop
H = heat received by drop by conduction
k = thermal conductivy of air (cal $cm^{-1}\ sec^{-1}\ deg^{-1}$)
$\phi = (\theta + T)/2$
a = drop radius

If there is significant relative motion between the drop and the medium let us write

$$\frac{dm}{dt} = -\frac{4\pi MDa}{R\phi}(p_T - fp_\theta)\, G_1 \quad . \quad . \quad . \quad . \quad (3)$$

$$\frac{dH}{dt} = 4\pi ka(\theta - T)\, G_2 \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where G_1 and G_2 are still to be determined. G_2 of course is the Nusselt number. If now we neglect any effects due to radiation and assume that the surface temperature of the drop is equal to the mean drop temperature (and that the density of water is unity) we have

$$\frac{4}{3}\pi a^3 \frac{dT}{dt} = 4\pi ka(\theta - T)\, G_2 - \frac{4\pi MDaL}{R\phi}(p_T - fp_\theta)\, G_1 \quad . \quad (5)$$

where L is the latent heat of vaporization of water. If we write V for the terminal velocity of the drop we have $dT/dt = \alpha V$ where α is the lapse rate of temperature of the drop (positive if T increases downwards) and

$$\alpha a^2 V = 3k(\theta - T)\, G_2 - \frac{3MDL}{R\phi}(p_T - fp_\theta)\, G_1 \quad . \quad . \quad (6)$$

It is physically obvious that α must exceed zero and it is equally obvious that α cannot exceed the lapse rate of θ. Later we shall need to assume a particular temperature structure for the atmosphere through which the drop falls and we shall select a structure with the I.C.A.N. lapse rate. We shall show (Table III below) that, if α is put equal to

the I.C.A.N. lapse rate, the term on the left-hand side of Eq. (5) is small compared with the terms on the right-hand side. The solution to Eq. (5) therefore depends largely upon the velocity coefficients G_1 and G_2.

In connection with the assumption that the mean drop temperature is equal to the surface temperature of the drop we may note that it can be shown that (e.g., see *Operational Methods in Applied Mathematics*, H. S. Carslaw and J. C. Jaeger, p. 235, Ex. 15) if a sudden temperature change is applied to the surface of a drop of water of 1 mm radius the temperature at the centre will have taken up 55 per cent of the change after 1 sec and 89·6 per cent after 2 sec. If the drop has a radius of 3 mm, 61·7 per cent of the change is communicated to the centre after 10 sec and 92·5 per cent after 20 sec.

3. The velocity coefficients G_1 and G_2

The experimental evidence available concerning the velocity coefficients G_1 and G_2 includes the well known works of Frossling (1938) and Takahashi (1936) on the evaporation of water drops and a summary by McAdams (1942) of the experimental work on the transfer of heat to or from a sphere moving in a fluid. It is clear also that the values of G_1 and G_2 should approach unity as Re approaches zero. Consideration of this experimental evidence led to the view that it is not practicable to distinguish between G_1 and G_2 and that, over the range of Re in which we are interested (up to 3,000), we may equate G_1 and G_2 to G where, as an average result of the experimental work mentioned above,

$$G = 1 + 0{\cdot}141\,(Re)^{0{\cdot}6} \quad . \quad . \quad . \quad . \quad . \quad (7)$$

After the computations described below had been completed a paper by Kinzer and Gunn (1951) was published describing new experimental work on the evaporation of water drops. Their results are not given in exactly the same form as adopted in the present paper but it was possible to deduce from their results values of G for various temperatures and drop sizes. This was done for the temperatures 15°C and 40°C and it was found that the ratio of the Kinzer and Gunn value to that given by Eq. (7) (for a pressure 1013 mb) decreased from about 1·13 at a drop diameter 0·5 mm to a minimum value of about 0·72 at a drop diameter about 2·25 mm and then increased again becoming 1·0 at a diameter of about 4·5 mm. (The different values for Re at lower pressures would increase these ratios somewhat). The scatter in the experimental values of G given by different workers is such that it was not considered justifiable to repeat the very laborious computations described below with a different formula for G which would take account of the new work by Kinzer and Gunn.

4. Method of computing variation of drop size with height

Let us now consider the variation in size, owing to evaporation, of a single drop as it falls through an atmosphere having specified characteristics. From Eq. (3) we easily obtain

$$a\frac{da}{dz} = \frac{MDG}{R\theta V}(p_T - fp_\theta) \quad . \quad . \quad . \quad . \quad (8)$$

in which θ has been written for ϕ as a reasonable approximation. (We shall see later in Table IV that this approximation is justifiable in the present connection). We have to evaluate the right-hand side of Eq. (8). Since the characteristics of the ambient

ir are specified we can determine D, θ, p_θ, and f. If the drop size is specified we can letermine G (from Eq. (7)) and V (see Best 1950b). It remains only to determine T and hence p_T) appropriate to the conditions.

The following procedure was adopted. After putting $G_1 = G_2 = G$ and writing for ϕ Eq. (6) can be rearranged to give

$$f = \frac{1}{p_\theta}\left[p_T - \frac{R\theta k}{MDL}\{(\theta - T) - \chi\}\right] \quad . \quad . \quad . \quad (9)$$

vhere

$$\chi = \frac{\alpha\, a^2\, \mathrm{V}}{3Gk} \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

For the specified conditions everything on the right-hand side of Eq. (9) is known the formulae used for the physical characteristics of air are given in the appendix) xcept T (and p_T) and α. We shall consider evaporation in an atmosphere with a apse rate of $6{\cdot}5 \times 10^{-5}$ °C cm^{-1} and shall also assume this value for α (the validity of his step is discussed more fully in Section 5 below). Thus, by assigning a series of values to T, we can calculate the corresponding values of f and hence by plotting the esults evaluate T for specified values of f. The value of p_T can then be obtained nd so $a\, da/dz$ determined from Eq. (8).

By means of this procedure $a\, da/dz$ was computed for the values of a and f shown n Table I, for $z = 0$, 1, 2, 3, 4 and 5 km in both the I.C.A.N. and summer tropical tmospheres (see appendix for specification of these atmospheres). Table I is typical f the results obtained which all followed the same general pattern.

TABLE I. VARIATION OF DROP RADIUS a WITH HEIGHT z OWING TO EVAPORATION

	Values of $10^{10}\, a\, da/dz$ (C.G.S.) in I.C.A.N. atmosphere at 2 km						
	Relative humidity ($100\, f$)						
(cm)	95	90	80	70	60	50	40
·015	5·25	10·7	23·5	35·4	46·5	59·5	72·0
·025	4·35	8·83	18·2	28·0	38·4	49·3	59·5
·035	4·07	8·26	17·0	26·2	36·0	46·1	55·8
·05	4·00	8·10	16·7	25·8	35·3	44·2	54·6
·10	2·97	7·53	17·0	27·0	37·6	47·5	58·9
·15	0·27	6·99	17·8	29·3	40·0	52·6	64·5
·20	− 1·43	4·49	16·8	29·9	43·6	56·5	70·1
·25	− 5·48	1·24	15·2	29·9	45·4	59·9	75·2
	Mean value above line						
	4·42	8·68	18·1	28·9	40·3	51·9	63·8

It can be seen that at low relative humidites $a\, da/dz$ is large for small values of , diminishes at first as a increases and then increases again. At high relative humidities owever the increase at large values of a does not occur. The most important variation f $a\, da/dz$ occurs as f varies.

The negative values in the bottom left-hand corner of the table occur as a result f the assumption that the lapse rate of drop temperature is equal to the lapse rate of ir temperature. When large drops fall through a nearly saturated atmosphere the vaporation is small and so the heat required to balance the loss of heat of vaporization s small. Owing to the size of the drop however the heat required to raise the drop

temperature as it falls into warmer air may be large. Thus the assumption that the left-hand side of Eq. (5) is small compared with each term on the right-hand side may no longer be justified. It is convenient at this stage to examine this point more closely before dealing with the results of computing $a\,da/dz$.

5. The lapse rate of drop temperature

It is not practicable to solve Eq. (5) without some assumption about the term dT/dt. It is of interest, however, to consider the case of a non-evaporating drop. The heat-balance equation is then Eq. (5) with the omission of the second term on the right-hand side. This reduces to

$$\frac{dT}{dz} = \frac{3kG}{a^2 V}(\theta - T)$$

If we put $\theta = \theta_0 + \beta z$ (measuring z downwards) and assume that the drop starts at a height $z = 0$ with temperature $T = \theta_0$ and that $kG/a^2 V$ is independent of z, the solution to this equation gives

$$\theta - T = \frac{\beta}{A}[1 - \exp(-Az)]$$

where

$$A = \frac{3kG}{a^2 V}$$

Calculation shows that A varies very slowly with z but very rapidly with a. Taking values appropriate to 2 km in the I.C.A.N. atmosphere we obtain Table II.

TABLE II. Data relating to a non-evaporating drop

a (cm)	0·015	0·05	0·1	0·15	0·2
$10^5 A$	1170	80	22·0	11·0	5·0
z (90) m	1·97	28·8	105·0	208·0	460·0
β/A (°C)	0·0055	0·081	0·29	0·59	1·3
Critical rel. hum.	100	99·4	97·9	95·9	91·1

The quantity $(\theta - T)$ reaches 90 per cent of its asymptotic value β/A after a fall of $2{\cdot}3/A$ cm. This quantity, in metres, is denoted by z (90) in Table II. Thus we see that a small drop very rapidly approaches a steady value for $(\theta - T)$ but a large drop falls a much greater distance before achieving a similar value. These figures apply to a non-evaporating drop; a drop of water falling in a nearly saturated atmosphere approaches this condition. The quantity β/A was denoted by χ when we were discussing an evaporating drop (Eq. 10) and computation shows that χ differs by less than 3 per cent in the I.C.A.N. and S.T. atmospheres. Its value increases with height, by about 25 per cent for a radius 0·015 cm and about 50 per cent for a radius 0·3 cm as the height increases from 0 to 5 km. The variation of β/A with drop size is extremely rapid and Table II shows some values appropriate to 2 km in the I.C.A.N. atmosphere.

We see from Table II that the difference between drop temperature and air temperature does not exceed 0·5°C unless the drop radius is about 1·5 mm or more. Thus

the lapse rate of drop temperature must approximate closely to the lapse rate of air temperature except for very large drops. In a non-saturated atmosphere it is not legitimate to use this argument to justify the assumption of identity between the air-temperature lapse rate and the drop-temperature lapse rate for an evaporating drop. However, we may still regard Eq. (6) with α replaced by the I.C.A.N. lapse rate as being a reasonable approximation to Eq. (5), provided the left-hand side is small in numerical value compared with each term on the right-hand side, i.e., provided the heat required to raise the drop temperature is small compared with the heat required to evaporate the water. This condition amounts to ensuring that $\chi/(\theta - T)$ is small. This quantity is tabulated in Table III for the conditions appropriate to Table I. The pattern of values is typical of other heights and of the summer tropical atmosphere.

TABLE III. RATIO OF HEAT REQUIRED TO RAISE TEMPERATURE OF AN EVAPORATING DROP TO HEAT SUPPLIED FROM AMBIENT AIR AS THE DROP FALLS INTO WARMER AIR

Values of $\chi/(\theta - T)$ for 2 km in the I.C.A.N. atmosphere

	Relative humidity ($100f$)						
a (cm)	95	90	80	70	60	50	40
0·015	0·014	0·0069	0·0037	0·0024	0·0017	0·0014	0·0011
0·025	0·047	0·023	0·012	0·0078	0·0058	0·0047	0·0038
0·035	0·097	0·049	0·024	0·016	0·012	0·0097	0·0079
0·05	0·20	0·10	0·051	0·034	0·025	0·020	0·016
0·10	0·59	0·33	0·17	0·12	0·089	0·070	0·059
0·15	0·84	0·59	0·33	0·23	0·17	0·14	0·11
0·20	1·16	0·77	0·46	0·33	0·26	0·21	0·17
0·25	1·30	0·93	0·59	0·43	0·34	0·28	0·23

Now in Eq. (9) we have not neglected the heat required to raise the temperature of the drop as it falls into warmer air; we have approximated to it. Accordingly, it is not necessary to restrict our argument to cases when $\chi/(\theta - T)$ is negligibly small. We need only ensure that the left-hand side of Eq. (6) is of less importance than the terms on the right-hand side. After consideration of these factors it was decided, arbitrarily, to accept all cases in which $\chi/(\theta - T)$ was less than 0·5. The stepped line in Tables I and III divides the acceptable cases from the remainder. It will be noted that, broadly, this means that the results will not be applicable to large drops falling through a nearly saturated atmosphere. At heights greater than 2 km the dividing line in Tables I and III was extended towards the bottom right-hand corner, drops of 0·25 cm radius being excluded at 50 per cent relative humidity at 5 km.

The data of Table II serve to demonstrate the fact that, owing to the lag of drop temperature behind ambient-air temperature as the drop falls into warmer air, evaporation may not occur even though the air is unsaturated. If the drop temperature is below the ambient dew point, condensation will take place instead of evaporation. The critical depression of the dew point is given by the values of β/A. The last line of Table II shows the relative humidity corresponding to these critical values of β/A. We see that the effect is negligible for very small drops but significant for large drops. The result of neglecting this effect is that the rate of evaporation of the larger drops, particularly in nearly saturated air, is over-estimated.

6. Variation with height of size of large drops

Having decided what part of Table I (and corresponding tables for other heights – not reproduced here) is free from significant error we shall now consider whether these data may be represented by a convenient formula which can be integrated to give change of drop size over a finite height interval. We see that in the acceptable region of Table I the variation of $a\,da/dz$ with a is small. Accordingly values above the stepped line were averaged. It is apparent that $a\,da/dz$ increases rapidly as a decreases below 0·015 cm and accordingly the average values should be used only to compute the fall distances for evaporation from some larger radius to 0·015 cm. We shall return to a consideration of small drops in Section 7.

Having obtained mean values of $a\,da/dz$ for each height, $\log_{10}(10^{10}\,a\,da/dz)$ was plotted against z for each value of f and for each standard atmosphere in Figs. 1 and 2. The plotted points approximate reasonably closely to families of parallel straight lines and for ease of computation there are advantages if these straight lines fit formulae of convenient type. The straight lines in Figs. 1 and 2 have been drawn from the formulae :

$$\text{I.C.A.N.} \quad 10^{10}\,a\,da/dz = 199{\cdot}5\,(1-f)^{1{\cdot}13}\exp(-2{\cdot}90\times10^{-6}\,z) \quad . \quad . \quad (11)$$

$$\text{S.T.A.} \quad 10^{10}\,a\,da/dz = 354{\cdot}8\,(1-f)^{1{\cdot}13}\exp(-1{\cdot}98\times10^{-6}\,z) \quad . \quad . \quad (12)$$

In some instances the line shown may not be the line of best fit for the plotted points. In order to separate the variables in Eqs. (11) and (12) it was necessary to treat the lines as a family and not individually. It can be seen that the plotted points approximate reasonably closely to these lines except perhaps above 4 km in the I.C.A.N. atmosphere.

It may appear strange that there should be so much scatter amongst the points corresponding to large values of f. The explanation is that $(\theta - T)$ is then small (e.g., about 0·7°C when $f = 0{\cdot}95$ in S.T.A.); but $(\theta - T)$ was obtained graphically and was read to only 0·1°C. Thus the error in determining $(\theta - T)$ may lead to a significant error in $(p_T - fp_\theta)$ and hence in $a\,da/dz$ for large values of f.

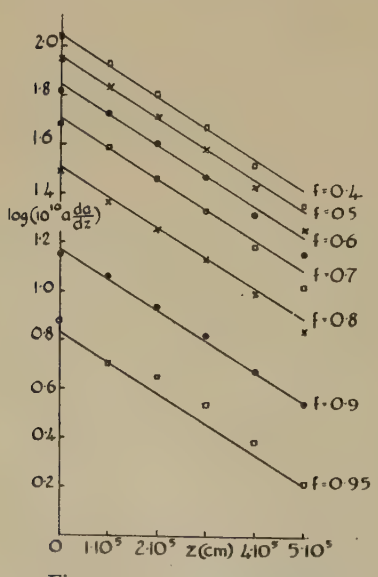


Figure 1. I.C.A.N. atmosphere.

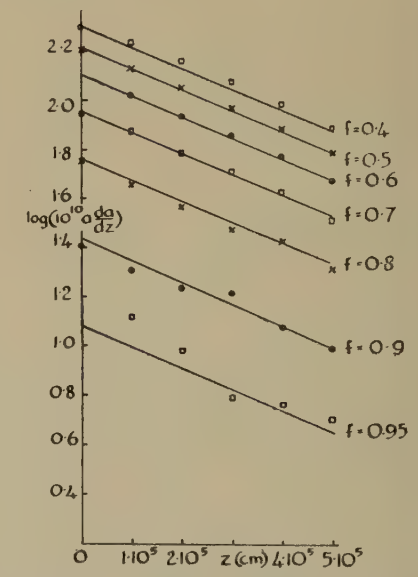


Figure 2. Summer tropical atmosphere.

Figures 1 and 2. Rate of variation of surface area of raindrop with height as a function of height.

7. Variation with height of size of small drops

We saw above (Table I) that $a\,da/dz$ increases as a diminishes below 0·015 cm and for that reason it was not justifiable to average $a\,da/dz$ when dealing with small drops. In passing we may note here that $a = 0{\cdot}015$ cm was also found (Best 1950b) a convenient size at which to divide the range of drop sizes when considering formulae for V. Since this size also corresponds to the generally accepted idea of the largest size for drizzle drops we shall refer to drops with $2a$ less than 0·3 mm by this term.

The procedure chosen for dealing with drizzle drops was based upon the fact that, as shown by the typical figures in Table III, the value of $\chi/(\theta - T)$ becomes very small for such drops. This was taken as justification for neglecting the term on the left-hand side of Eq. (6) and evaluating $(\theta - T)$ on the assumption that the heat required to raise the drop temperature as it falls into warmer air can be ignored in comparison with the heat required to balance the evaporative loss. We see then from Eq. (6) that $(\theta - T)$ will be independent of drop size. In fact the computations for the larger drops had already shown that $(\theta - T)$ never varied by more than 0·1°C as a varied from 0·015 to 0·05 cm. Accordingly the values of $(\theta - T)$ already computed for $a = 0{\cdot}015$ cm were used for drizzle drops. As a matter of interest these values are given in Table IV. In passing it may also be of interest to note that as the drop size increases to large raindrop size in the I.C.A.N. atmosphere these values might increase by 20 per cent or 10 per cent at very high or very low humidities at the surface and by nearly 50 per cent at all humidities at 5 km. In the S.T.A. the corresponding figures were 10 per cent and 2 to 3 per cent at the surface, and 10 per cent at 5 km.

TABLE IV. Depression of drop temperature (°C) below air temperature for an evaporating drop of radius 0·015 cm

	Relative humidity (100 f)						
z (km)	95	90	80	70	60	50	40
				I.C.A.N.			
0	0·5	1·0	2·1	3·3	4·5	5·7	7·0
1	0·5	0·9	1·9	2·8	3·8	4·9	5·9
2	0·4	0·8	1·5	2·3	3·2	4·0	4·9
3	0·3	0·6	1·3	1·9	2·6	3·2	3·9
4	0·2	0·5	1·0	1·5	2·0	2·5	3·1
5	0·2	0·4	0·7	1·1	1·5	1·9	2·3
				S.T.A.			
0	0·8	1·7	3·5	5·5	7·7	10·1	12·8
1	0·7	1·6	3·3	5·1	7·1	9·3	11·6
2	0·7	1·5	3·0	4·7	6·5	8·5	10·5
3	0·7	1·3	2·8	4·3	5·9	7·6	9·4
4	0·6	1·2	2·4	3·8	5·2	6·7	8·3
5	0·5	1·1	2·2	3·3	4·6	5·9	7·2

The air temperature at the surface in the I.C.A.N. atmosphere (15°C) is the same as the air temperature at 4 km in the S.T.A. The different values of $(\theta - T)$ for these two conditions are attributable to the effect of barometric pressure on D.

Returning to the problem of finding an integrable formula for the rate of change of size with height of a drizzle drop we see that Eq. (8) can be written

$$a\frac{da}{dz} = \frac{M}{R}\frac{D\,(p_T - fp_\theta)}{\theta}\frac{G}{V}$$

and that in this equation the first factor on the right-hand side is an absolute constant and the second factor is independent of a. The third factor, G/V, depends upon both z and a but is independent of f. Following inspection of the data and in order to separate the variables a and z in the factor G/V, this equation was rewritten in the form

$$10^{10}\, a \frac{da}{dz}\left(\frac{G}{V}\right)_{0\cdot015}\Big/\frac{G}{V} = \frac{M}{R}\,\frac{10^{10}\, D\,(p_T - f p_\theta)}{\theta}\left(\frac{G}{V}\right)_{0\cdot015} \quad . \quad . \quad . \quad (13)$$

where $(G/V)_{0\cdot015}$ denotes the value of G/V for drops of radius 0·015 cm and varies with z. The right-hand side of Eq. (13) was computed for various heights and values of f in both the standard atmospheres. The results are shown plotted in Figs. 3 and 4. The straight lines which have been fitted to the plots were computed from the formulae :

I.C.A.N. $$10^{10}\, a \frac{da}{dz}\left(\frac{G}{V}\right)_{0\cdot015}\Big/\frac{G}{V} = 214\cdot8\,(1-f)^{1\cdot062}\exp\,(-2\cdot79\times10^{-6}\,z) \quad . \quad (14)$$

S.T.A. $$10^{10}\, a \frac{da}{dz}\left(\frac{G}{V}\right)_{0\cdot015}\Big/\frac{G}{V} = 386\cdot4\,(1-f)^{1\cdot062}\exp\,(-1\cdot74\times10^{-6}\,z) \quad . \quad (15)$$

It can be seen that these lines are reasonably good fits except perhaps above 4 km at low humidities in the I.C.A.N. atmosphere.

The next step was to evaluate $(G/V)_{0\cdot015}/(G/V)$. This quantity varies with drop size and with height but it was found that the variation with z was negligible. The variation from 0 to 5 km was about 5 per cent for $a = 0\cdot0025$ cm in both atmospheres and about 1 per cent for $a = 0\cdot0125$ cm. Accordingly the values were meaned for all heights and finally these mean values were used to plot Fig. 5. It can be seen that for drop radii from 0·015 cm to 0·005 cm the difference between the two atmospheres is negligible. The straight line drawn in Fig. 5 represents the relation

$$\frac{G}{V} = \left(\frac{G}{V}\right)_{0\cdot015}\frac{0\cdot0145}{a} \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

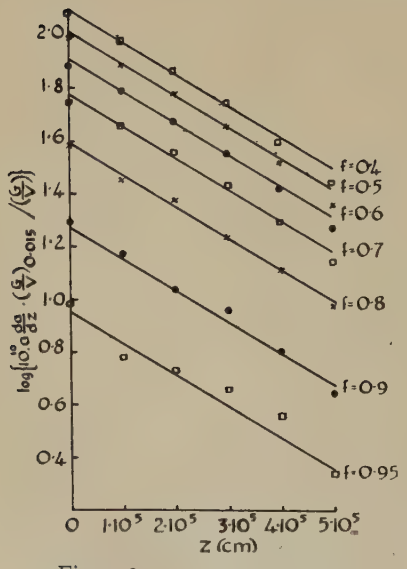


Figure 3. I.C.A.N. atmosphere.

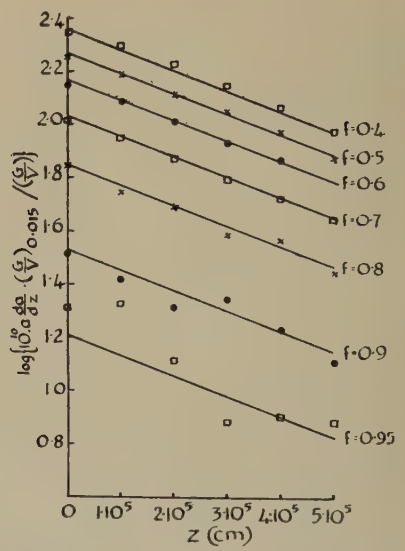


Figure 4. Summer tropical atmosphere.

Figures 3 and 4. Rate of variation of surface area of drizzle drop with height as a function of height.

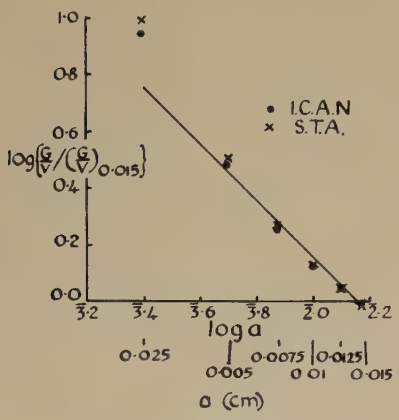


Figure 5. Variation of G/V with drop size.

and is clearly a good approximation for drop radii from 0·015 cm to 0·005 cm. Below this size the index of a in the denominator on the right-hand side of Eq. (16) should be increased but the drops are now becoming of the size of cloud particles and are outside the range of consideration of the present paper.

If we combine Eqs. (16) with (14) and (15) we obtain

I.C.A.N. $$10^{10}\, a^2 \frac{da}{dz} = 3{\cdot}115\,(1-f)^{1{\cdot}062} \exp\,(-2{\cdot}787 \times 10^{-6}\, z) \quad . \quad . \quad . \quad (17)$$

S.T.A. $$10^{10}\, a^2 \frac{da}{dz} = 5{\cdot}603\,(1-f)^{1{\cdot}062} \exp\,(-1{\cdot}741 \times 10^{-6}\, z) \quad . \quad . \quad . \quad (18)$$

These two equations correspond to Eqs. (11) and (12) for large raindrops.

8. Graphs of variation of drop size with height

The computations described above were deliberately framed to produce, if possible, formulae which could be easily integrated to give the change of drop size with height. From Eqs. (11) and (12), (17) and (18) we see that, if a drop of radius a_1 at height z_1 evaporates to a radius of a_2 at height z_2, the relation between the four quantities is given by Eqs. 19 to 22.

Raindrops

I.C.A.N. $$a_1^2 - a_2^2 = 137{\cdot}5 \times 10^{-4}\,(1-f)^{1{\cdot}13} \left[\exp\,(-2{\cdot}90 \times 10^{-6}\, z_2) - \exp\,(-2{\cdot}90 \times 10^{-6}\, z_1)\right] \quad . \quad (19)$$

S.T.A. $$a_1^2 - a_2^2 = 358{\cdot}4 \times 10^{-4}\,(1-f)^{1{\cdot}13} \left[\exp\,(-1{\cdot}98 \times 10^{-6}\, z_2) - \exp\,(-1{\cdot}98 \times 10^{-6}\, z_1)\right] \quad . \quad (20)$$

Drizzle drops

I.C.A.N. $$a_1^3 - a_2^3 = 3{\cdot}35 \times 10^{-4}\,(1-f)^{1{\cdot}062} \left[\exp\,(-2{\cdot}79 \times 10^{-6}\, z_2) - \exp\,(-2{\cdot}79 \times 10^{-6}\, z_1)\right] \quad . \quad (21)$$

S.T.A. $$a_1^3 - a_2^3 = 9{\cdot}65 \times 10^{-4}\,(1-f)^{1{\cdot}062} \left[\exp\,(-1{\cdot}74 \times 10^{-6}\, z_2) - \exp\,(-1{\cdot}74 \times 10^{-6}\, z_1)\right] \quad . \quad (22)$$

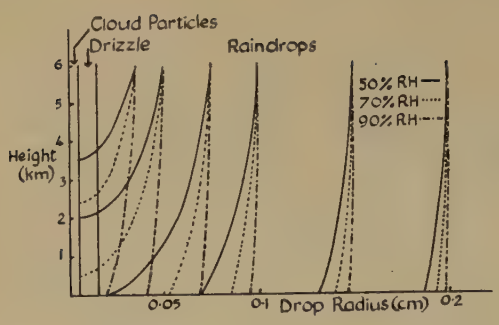


Figure 6. In I.C.A.N. atmosphere.

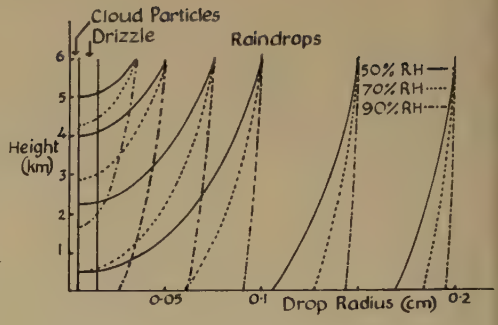


Figure 7. In summer tropical atmosphere.

Figures 6 and 7. Variation of drop size with height.

The evaporative process must be considered in two stages, of course, separated by the height at which the radius is 0·015 cm. Furthermore Eqs. (21) and (22) must not be applied to drops smaller than 0·005 cm radius. Eqs. (19) to (22) have been used to construct the curves in Figs. 6 and 7. These show the variation of drop size with height for drops of six different sizes starting to fall from 6 km through either the I.C.A.N. or S.T. atmosphere and with a relative humidity of either 50, 70 or 90 per cent. Curves for other sizes, starting heights or humidities obviously can be interpolated.

Although we are not concerned here with the evaporation of cloud particles we may note that Fig. 5 shows that for such small drops the index of the radius on the left-hand side of Eqs. (21) and (22) would be greater than three and the fall distance for evaporation would be correspondingly diminished.

Findeisen (1939) has considered the evaporation of raindrops using Frossling's data for the rate of evaporation of large drops, psychrometric theory to determine the temperature of the drop and a rather inaccurate formula suggested by Schmidt for the terminal velocity of water drops. Findeisen computes the fall distance for evaporation of a single drop for only one set of ambient conditions and apparently without specifying the ambient lapse rate of temperature so that comparison with the present work is rather difficult. It does appear, however, that the order of magnitude of the answer he obtained does not differ from the present result.

PART II.—The evaporation of rain in a constant atmosphere

9. Effect of evaporation on drop-size distribution and liquid water concentration in rain

In the preceding sections we have been concerned with the evaporation of a single drop of water as it falls through either the I.C.A.N. or summer tropical atmosphere in which the humidity has a specified value. Rain is characterized by the size and size distribution of the drops and the number of drops per unit volume of air (or alternatively the amount of liquid water per unit volume of air). We propose now to consider the effect of evaporation upon these characteristics of rain and, in this part of the work, we shall assume that the evaporation of the drops does not affect the characteristics of the ambient air.

We have seen that the amount by which a drop evaporates while falling through the atmosphere depends upon the length of the fall path, the ambient temperature and humidity and the pressure. Eqs. (19) and (20) show that the change in surface area of a drop as it falls a specified distance through the air is a function *only* of these characteristics of the air and of the heights at the beginning and end of the fall, that is the change in surface area is independent of the drop size. (We shall ignore the implications of Eqs. (21) and (22) since only a small amount of water in rain is associated with drops of radius less than 0·015 cm). If then we write

$$a_1^2 - a_2^2 = K^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

we see that K is the initial radius of the drop which, according to Eqs. (19) and (20), would just evaporate completely while falling between specified heights through one of the standard atmospheres having a specified relative humidity. In fact K is a convenient measure of the amount of evaporation which will take place over a specified fall path. We shall call K the ' evaporation radius ' corresponding to a particular fall path through an atmosphere of specified characteristics. The numerical value of K is obtained by equating K^2 to the right hand side of Eqs. (19) or (20) as appropriate. It is useful to examine the order of magnitude of K. If, in Eq. (20), we put $f = 0{\cdot}5$, $z_1 = 6$ km and $z_2 = 0$ km we get $K^2 = 0{\cdot}01139$ and so K varies from 0 to about 0·1 cm.

It is obvious that the effects of evaporation upon the size and size distribution of the collection of drops constituting rain will depend upon the initial drop-size distribution. Let us suppose that at the initial height the drop-size distribution is given by

$$1 - F = \exp\left[-(2a/b)^n\right] \quad . \quad . \quad . \quad . \quad . \quad (24)$$

where F is the fraction of liquid water in the air composed of drops with radius less than a, and b and n are constants. It has been shown elsewhere (Best 1950a) that this form of distribution is in reasonable agreement with measurements at ground level. We shall also use the symbol W to denote the liquid-water concentration (g cm^{-3}) at the initial height. We notice that b has the dimensions of a length. In fact the value of n determines the shape of the distribution curve represented by Eq. (24) but the value of b determines the drop size about which the sizes are distributed. We may regard b as a scale diameter for the distribution represented by Eq. (24). We have dealt with this matter in some detail because, as we shall see below, the effects of evaporation on rain are conveniently measured not in terms of the evaporation radius K but rather in terms of the ratio of K to the scale diameter b. For any specified fall path and value of b the ratio is easily evaluated, as already explained, with the help of Eqs. (19) or (20) and (23).

The problem now is to determine the drop-size distribution in the rain, and the liquid-water concentration, at some variable level below the initial height where the drop-size distribution was given by Eq. (24) and the liquid-water concentration was W. The variable level is specified by the value of K, i.e., it is the level at which the surface area of a drop, starting from the initial height, will have decreased by $4\pi K^2$. It is easily shown from Eqs. (23) and (24) that at some height, less than the initial height and where the evaporation radius has a value K

$$\text{Total volume of drops with radius less than } (r^2 - K^2)^{\frac{1}{2}} = Wn\left(\frac{2}{b}\right)^n \int_K^r a^{n-1}(a^2 - K^2)^{3/2} \exp\left[-(2a/b)^n\right] da \quad . \quad (25)$$

In Eq. (25) the fall path of the drops and the characteristics of the atmosphere are represented by the presence of the parameter K. At the level K let $W(K)$ denote the liquid-water concentration and let $F(r, K)$ denote the fraction of liquid water comprised by drops with radius $< r$.

Then from Eq. (25) we easily get

$$W(K) = Wn\, 2^n \int_{K/b}^{\infty} H(u)\, du \quad . \quad . \quad . \quad . \quad (26)$$

$$F(r, K) = \int_{K/b}^{\left(\frac{r^2}{b^2}+\frac{K^2}{b^2}\right)^{1/2}} H(u)\, du \Big/ \int_{K/b}^{\infty} H(u)\, du \quad . \quad . \quad (27)$$

where

$$H(u) = u^{n-4}\left(u^2 - \frac{K^2}{b^2}\right)^{3/2} \exp\left[-(2u)^n\right] \quad . \quad . \quad (28)$$

We note that K and b occur in these expressions only in the form of the ratio K/b. Thus, if we express the evaporation radius K as a fraction of the initial scale diameter b, the liquid-water concentration $W(K)$ and the drop-size distribution function $F(r, K)$ will be independent of b. It has been shown (Best 1950a) that b is of the order 0·1 to 0·3 cm so that K/b varies from 0 to 1. We have seen that K is a convenient measure of the fall path of the drops over which the evaporation takes place. The ratio K/b is an even more convenient measure of the length of the fall path since it absorbs the scale diameter b which is characteristic of the initial drop-size distribution.

The integrals in Eq. (27) cannot be evaluated explicitly. They have been computed graphically for a selection of values of n and of K/b.

Let us suppose that at a height where the evaporation radius is K the drop-size distribution has the same general form as that represented by Eq. (24). Let us write

$$1 - F(r, K) = \exp\left[-(2r/bc)^m\right] \quad . \quad . \quad . \quad (29)$$

i.e., we suppose the scale diameter has changed from b to bc and the distributive index from n to m. Then we have

$$\log\log\left\{1/[1 - F(r, K)]\right\} = \log\log e + m\left[\log(2r/b) - \log c\right] \quad . \quad (30)$$

where all the logarithms are to base 10. Thus, if our supposition about the form of the drop distribution is correct, we should get a straight line by plotting $\log\log\left\{1/[1 - F(r, K)]\right\}$ against $\log(2r/b)$ and from the constants of this straight line we should be able to evaluate m and c. This procedure has been followed using values of $F(r, K)$ computed graphically (Eq. 27). The results are shown in Figs. 8 (a)-(d) for four values of n.

In the interests of clarity it was thought better to plot the computed points in Fig. 8 (b) and to draw the line only for $K/b = 0$. For the same reason the scale of Figs. 8 (b), (c) is different from that adopted for Figs. 8 (a), (d).

The lines in Figs. 8 (a)-(d) are substantially straight if we disregard the 10 per cent of water comprised by the smallest drops and the 10 per cent comprised by the largest drops, that is for values of $\log\log\left\{1/[1 - F(r, K)]\right\}$ between $\bar{2}{\cdot}66$ and 0. Accordingly the best straight line has been fitted in each case by the method of least squares. For this purpose five points were interpolated on each curve for equally spaced values of the ordinate between $\bar{2}{\cdot}66$ and 0·0 inclusive. From these straight lines the values of m and c were determined. These are tabulated in Tables V and VI.

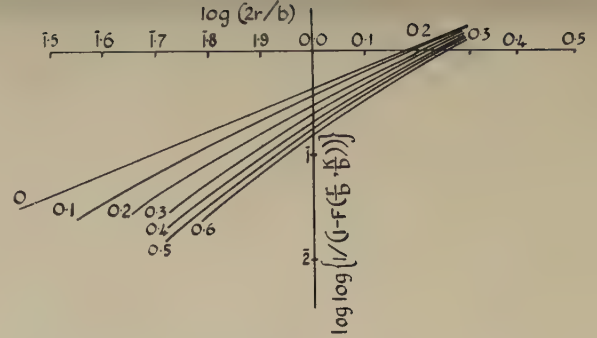


Figure 8a. $n = 2$. (The figures on the lines are the values of K/b).

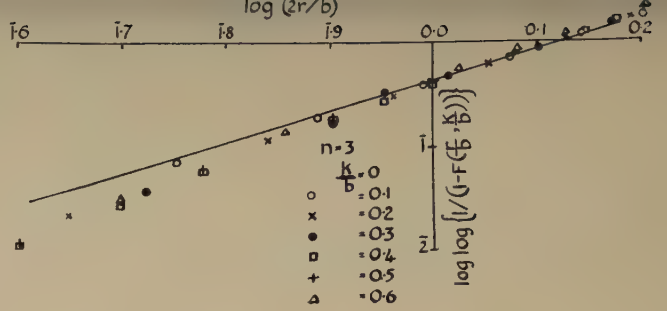


Figure 8b. $n = 3$. (The plotted points are computed for various values of K/b according to the key).

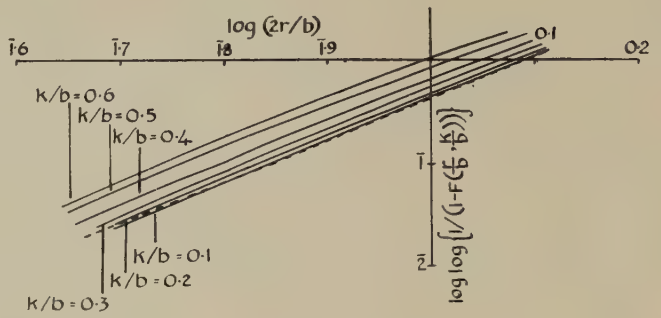


Figure 8c. $n = 4$. (The figures on the lines are the value of K/b). The broken line almost coincident with the line for $K/b = 0{\cdot}1$ is the line for $K/b = 0$.

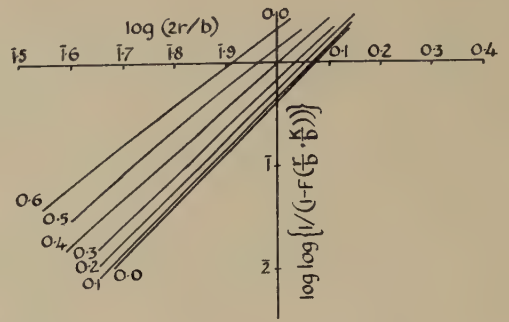


Figure 8d. $n = 5$. (The figures on the lines are the values of K/b).

Figures 8a – 8d. Effect of evaporation on drop-size distribution.

It will be appreciated that graphical evaluation of $F(r, K)$ inevitably leads to the introduction of small errors which are difficult to estimate. This probably accounts for small irregularities in Tables V and VI. Obviously when $K/b = 0$ we have $m = n$ and $c = 1$.

TABLE V. VALUE OF DISTRIBUTIVE INDEX m IN DROP-SIZE DISTRIBUTION AFTER DROPS OF RADIUS K HAVE JUST EVAPORATED. b IS INITIAL SCALE DIAMETER

	Initial distributive index n			
	2	3	4	5
K/b	Values of m			
0·0	2	3	4	5
0·1	2·38	3·13	4·05	4·91
0·2	2·68	3·45	4·01	4·78
0·3	2·98	3·59	4·05	4·73
0·4	3·11	3·58	3·99	4·36
0·5	3·23	3·56	3·92	4·16
0·6	3·36	3·68	4·01	3·83

TABLE VI. RATIO OF SCALE DIAMETER IN DROP-SIZE DISTRIBUTION TO INITIAL VALUE AFTER DROPS OF RADIUS K HAVE JUST EVAPORATED. b IS INITIAL SCALE DIAMETER

	Initial distributive index n			
	2	3	4	5
K/b	Values of c			
0·0	1	1	1	1
0·1	1·09	1·01	1·00	0·99
0·2	1·16	1·03	0·98	0·96
0·3	1·24	1·03	0·94	0·90
0·4	1·30	1·03	0·90	0·83
0·5	1·34	1·03	0·84	0·74
0·6	1·40	0·99	0·79	0·65

We see from these tables that the variations of the distributive index m depend very much upon the initial value. Broadly it appears that the effect of evaporation is to bring m to a value between 3·5 and 4·0. It should perhaps be remarked that the calculations mentioned were rather laborious and those for higher values of K/b were probably less accurate than those for low values.

From Table VI we get the rather surprising result that the scale diameter, bc, is increased by evaporation for small values of the distributive index but is decreased for large values of n. It has been shown (Best 1951) that with drop-size distribution represented by Eq. (24) the ratio of the mean diameter to bc, whatever reasonable interpretation is placed upon the word ' mean,' depends upon m and may depend upon the smallest measurable size. This ratio, however, is always independent of bc and increases as m increases. Now considering Tables V and VI together we see that if m has an initial value 2, both m and bc increase with K/b and so the mean diameter increases. If m has an initial value 5, both m and bc decrease as K/b increases and so the mean diameter decreases. For the intermediate cases of $n = 3$ or 4, m increases with bc practically constant or m is almost constant but with decreasing bc. Thus for $n = 3$ the mean diameter increases and with $n = 4$ the mean diameter decreases. In brief then evaporation increases the mean diameter if $n = 2$ or 3 and decreases it if $n = 4$ or 5.

The liquid-water concentration corresponding to evaporation radius K is given by Eq. (26). The values of $W(K)/W$ have been computed and are given in Table VII. Here again we see that early evaporation of the small drops in a wide distribution (n small) causes an early reduction in W but subsequently the large drops in the wide distribution slow down the total rate of evaporation. It is rather interesting that $W(K)/W$ is independent of n when K/b has a value about 0·35.

TABLE VII. RATIO OF LIQUID-WATER CONCENTRATION IN RAIN TO INITIAL VALUE AFTER DROPS OF RADIUS K HAVE JUST EVAPORATED. b IS INITIAL SCALE DIAMETER

	Initial distributive index n			
	2	3	4	5
K/b	Values of $W(K)/W$			
0·0	1	1	1	1
0·1	0·81	0·88	0·90	0·92
0·2	0·56	0·62	0·67	0·68
0·3	0·35	0·37	0·38	0·40
0·4	0·20	0·18	0·17	0·16
0·5	0·11	0·069	0·050	0·038
0·6	0·052	0·021	0·0089	0·0037

10. EFFECT OF EVAPORATION UPON RADAR RESPONSE FROM FALLING RAIN

The radar response from a cloud of falling drops is proportional to Σa^6 where the summation is taken over all the drops in unit volume. If the drop-size distribution after evaporation is represented by Eq. (29) it is easily shown that

$$\Sigma a^6 = \frac{3\,W(K)\,b^3\,c^3}{32\,\pi}\,\Gamma\left(\frac{m+3}{m}\right) \qquad (31)$$

If now we write $Z(K)$ for the value of Σa^6 after an evaporation path K/b, and Z for the same quantity at the initial height we have, using the nomenclature of the preceding section,

$$\frac{Z(K)}{Z} = \frac{W(K)\,c^3\,\Gamma\left(1+\frac{3}{m}\right)}{W\,\Gamma\left(1+\frac{3}{n}\right)} \qquad (32)$$

Using the numerical values from Tables V, VI and VII, Table VIII has been computed to show the effect of evaporation on radar response.

TABLE VIII. RATIO OF RADAR RESPONSE FROM RAIN TO INITIAL VALUE AFTER DROPS OF RADIUS K HAVE JUST EVAPORATED. b IS INITIAL SCALE DIAMETER

	Initial distributive index n			
	2	3	4	5
K/b	Values of $Z(K)/Z$			
0·0	1	1	1	1
0·1	0·899	0·892	0·898	0·893
0·2	0·701	0·645	0·630	0·604
0·3	0·503	0·381	0·315	0·293
0·4	0·325	0·186	0·124	0·093
0·5	0·194	0·0711	0·0298	0·0157
0·6	0·103	0·0190	0·0044	0·00105

We see that the slow rate of evaporation of the large drops in a wide distribution more than balances the rapid evaporation of the small drops and leads to a slower rate of decrease of Z than occurs in rain composed of more uniform drops.

11. Effect of evaporation upon rate of precipitation

In Section 9 we considered the effect of evaporation upon the concentration of liquid water in the air. The rate of precipitation, i.e., the rate at which rain falls on a horizontal surface, is a function both of the liquid-water concentration and of the drop-size distribution and since evaporation affects both these characteristics it is not immediately obvious how the rate of precipitation will be affected. If the liquid water concentration and the drop-size distribution are known the rate of precipitation can be computed from a knowledge of the terminal velocities of drops of different sizes. It is clear also that if the drop-size distribution is constant the rate of precipitation will be directly proportional to the liquid-water concentration.

If the drop-size distribution is defined by Eq. (24) it is easily shown that

$$I = 7{\cdot}2 \times 10^{-2} \frac{n}{b^n} W \int_0^\infty \mathrm{V}\,(2a)^{n-1} \exp\left[-(2a/b)^n\right] da \; . \qquad (33)$$

where I is the rate of precipitation in mm/hr, W is the liquid-water concentration in g/m^3 and V is the terminal velocity (cm/sec) of a drop of radius a. The integral in the right-hand side of Eq. (33) has been evaluated graphically for a selection of values of b and n using the values of V appropriate to the surface in a summer tropical atmosphere (Best 1950b). Values of I/W are given in Table IX.

TABLE IX. Ratio of rate of precipitation (mm/hr) to liquid-water content of air (g/m^3)

n / b (cm)	2	3	4	5
0·05	6·2	6·3	6·5	6·5
0·10	11·9	12·2	12·5	12·8
0·15	16·5	17·2	17·5	17·7
0·20	19·9	20·9	21·4	22·0
0·25	22·6	24·0	24·5	25·0

Table IX demonstrates that the influence of n on I/W is practically negligible but that the ratio I/W is very materially affected by the value of b. From Tables V, VI, VII and IX it is possible to compute the effect of evaporation on the rate of precipitation in any specified case.

For subsequent use the values in Table IX have been averaged for all values of n and $\log I/W$ has been plotted against $\log b$ in Fig. 9. The plotted points depart but little from a straight line over the range of b from 0·1 to 0·25. It has been shown (Best 1950b) that this is a reasonable range of values for b and the straight line in Fig. 9 has accordingly been drawn ignoring the point for $b = 0{\cdot}05$ cm. The equation to this line is

$$I = 67\, W\, b^{0{\cdot}73}$$

In a discussion of measurements of the size distribution of raindrops (Best 1950a) it was shown that b and W are proportional to I raised to some power. Using the numerical

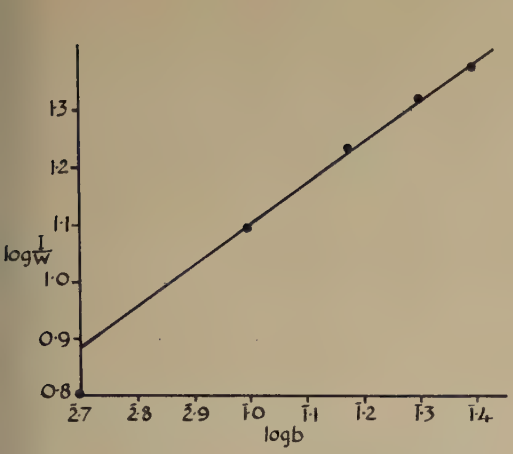


Figure 9. Effect of drop-size distribution on ratio of rate of precipitation to liquid-water content of air.

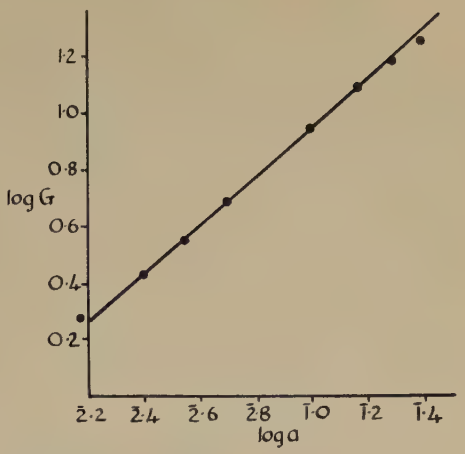


Figure 10. Relation between velocity factor G and drop radius a when drop is falling through atmosphere.

values of the constants given in that paper (and which apply primarily to temperate latitudes) it can be shown that, after making allowance for the different units employed here,

$$I = 58\, W b^{0\cdot 664}$$

is in reasonable agreement with the computed result now obtained.

PART III.—The effect of the evaporation of rain on the ambient air

12. Basic equations

So far we have dealt only with the evaporation of drops into an atmosphere the characteristics of which are not affected by the evaporation. In practice, of course, the temperature and humidity of the air are influenced, sometimes largely, by the evaporation of raindrops falling through the air. As the drops evaporate their temperature falls and heat is then supplied to the drops from the air, thereby lowering the air temperature. The evaporation of the drops also increases the humidity of the air and this in turn reduces the rate of evaporation. We shall now consider the variation with time of the temperature and humidity of the air through which rain is falling. For simplicity we shall confine this part of the work to conditions at the surface.

Let the number of drops per unit volume of air and having radius between a and $a + da$ be $N\,da$. The rate of gain of heat by each drop is (Eq. (4)), $4\pi ka\,(\theta T)\,G$. Integrating this over all the drops and equating to the rate of loss of heat from the air we get

$$c_p\,\sigma\,\frac{d\theta}{dt} = -\int_0^\infty 4\pi ka\,(\theta - T)\,GN\,da \quad . \quad . \quad . \quad (34)$$

where c_p is the specific heat of air at constant pressure and σ is the air density.

If ρ is the ambient vapour density we obtain in a similar way (Eq. (3)) an equation representing the conservation of water, viz.,

$$\frac{d\rho}{dt} = \int_0^\infty \frac{4\pi MDa}{R\theta}(p_T - fp_\theta)\, GN\, da \quad . \quad . \quad . \quad (35)$$

The term ϕ in Eq. (3) has been replaced by θ as a close approximation.

Eqs. (34) and (35) are the basic equations of temperature and humidity change. Before we can solve these equations we must examine the two integrals.

13. Relation between air temperature and vapour density, owing to evaporation of rain

Let us consider the quantity, $(\theta - T)$. As already indicated in Section 7, $(\theta - T)$ is almost independent of drop size. Table X is given to illustrate this.

TABLE X. Depression of temperature of evaporating drops below air temperature at a pressure 1013·2 mb and for an air temperature 301°A

	Relative humidity ($100f$)						
a (cm)	95	90	80	70	60	50	40
0·015	0·7	1·4	2·9	4·5	6·15	8·0	9·9
0·025	0·7	1·4	2·9	4·5	6·2	8·0	9·95
0·035	0·7	1·4	2·95	4·5	6·2	8·0	9·95
0·05	0·7	1·45	2·95	4·55	6·2	8·0	9·95
0·10	0·75	1·5	3·0	4·6	6·25	8·1	10·0
0·15	0·85	1·55	3·05	4·65	6·3	8·15	10·1
0·20	0·90	1·6	3·1	4·7	6·35	8·2	10·15
0·25	0·95	1·65	3·2	4·8	6·45	8·3	10·2

The values in the bottom left-hand corner of the table correspond to $\chi > 0{\cdot}5\,(\theta - T)$. Excluding these cases we see that there is very little variation in $(\theta - T)$ with a and that we can justifiably take mean values. Similar results were obtained at other temperatures from 288°A to 314°A. Table XI shows the mean values of $(\theta - T)$ for values of a up to and including 0·25 cm and for which $\chi < 0{\cdot}5\,(\theta - T)$ for a pressure 1013·2 mb and various temperatures.

TABLE XI. Mean values of depression of temperature of evaporating drops below air temperature $(\theta - T)$

	Relative humidity ($100f$)						
θ (°A)	95	90	80	70	60	50	40
288	0·52	1·10	2·24	3·42	4·63	5·84	7·15
294·5	0·63	1·27	2·63	3·98	5·44	6·97	8·57
301	0·71	1·47	3·01	4·60	6·26	8·09	10·02
307·5	0·80	1·62	3·34	5·09	7·04	9·13	11·43
314	0·86	1·73	3·56	5·57	7·75	10·16	12·89

(In passing we may note here that Byers, Moses and Harney (1949) state that the rain temperature may initially be up to 10°C lower than the ambient air temperature. The conditions they were discussing – summer thunderstorms in Ohio – would correspond to $\theta \simeq 302$°A and $f = 0{\cdot}5$ to 0·7. The initial temperature difference presumably

occurs, at least sometimes, before downdraught has started and from Table XI we should expect $(\theta - T)$ to be 5° to 8°C in such circumstances.)

Now if $(\theta - T)$ is independent of a it follows that $(p_T - fp_\theta)$ is also independent of a. Accordingly Eqs. (34) and (35) can be written in the form

$$\frac{d\theta}{dt} = -\frac{4\pi}{c_p}\frac{k}{\sigma}(\theta - T)\,X \quad . \quad . \quad . \quad . \quad . \quad (36)$$

$$\frac{d\rho}{dt} = \frac{4\pi M}{R}\frac{D}{\theta}(p_T - fp_\theta)\,X \quad . \quad . \quad . \quad . \quad . \quad (37)$$

$$X = \int_0^\infty a\,GN\,da \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

and it follows from Eqs. (36) and (37) that

$$\frac{d\theta}{dt} = -\frac{R}{c_p M}\left[\frac{k\theta\,(\theta - T)}{\sigma D\,(p_T - fp_\theta)}\right]\frac{d\rho}{dt}$$

The quantity $k\theta\,(\theta - T)/\sigma\,D\,(p_T - fp_\theta)$ was evaluated for each case tabulated in Table XI. The results showed some systematic variation with both f and θ but the variation was small. The mean of the 35 values was 0·1176 with a mean absolute deviation of 0·0062 and a standard deviation of 0·0080. Accordingly the mean value of 0·1176 was adopted and so it followed that

$$\frac{d\theta}{dt} = -\frac{0{\cdot}1176\,R}{c_p M}\frac{d\rho}{dt}$$

Putting in numerical values for the constants and integrating we get

$$\theta = A' - 2{\cdot}263 \times 10^6\,\rho \quad . \quad . \quad . \quad . \quad . \quad (39)$$

where A' is a constant which depends upon the initial conditions.

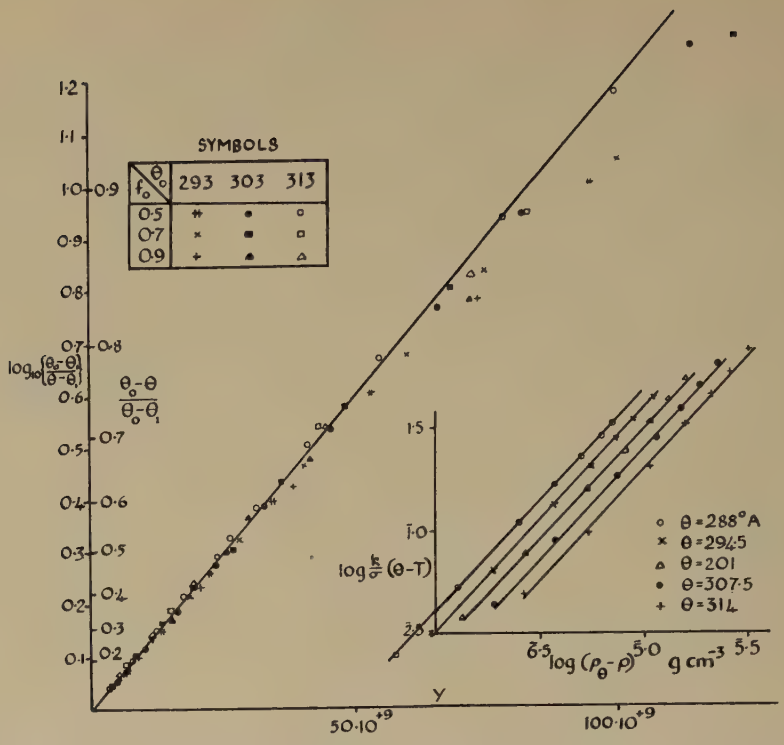
14. Relation between $(\theta - T)$ and θ

In order to determine the variation of θ with time we must integrate Eq. (36). Now X is a function only of the drop-size distribution and of the number of drops, i.e., it is independent of θ and t provided G is independent of θ. We shall show below that this is approximately true. Thus, in order to integrate Eq. (36), we must express $k\,(\theta - T)/\sigma$ in terms of θ.

In Table XI we have expressed $(\theta - T)$ as a function of θ and of f. Now f can be expressed in terms of ρ and ρ_θ, the saturation value of ρ at temperature θ. Moreover ρ_θ is a function only of θ and, from Eq. (39), we see that ρ is also a function of θ. Since k/σ is also a function of θ (for the given atmospheric pressure 1013·2 mb) we see that $k\,(\theta - T)/\sigma$ can be expressed as a function of θ and Eq. (36) can then be integrated. Examination of the numerical values of $k\,(\theta - T)/\sigma$ suggested that this quantity could be expressed in the form

$$\frac{k}{\sigma}(\theta - T) = 1{\cdot}905 \times 10^9\,(\rho_\theta - \rho)^{1{\cdot}064}\exp(-0{\cdot}0343\,\theta) \quad . \quad . \quad (40)$$

That this is so is shown in Fig. 11 where the plotted points have been computed from the values in Table XI and the straight lines have been computed from Eq. (40). From



(Inset) Figure 11. Drop-temperature depression as function of vapour-density deficit (below saturation) in ambient air.

Figure 12. Variation of air temperature with time as rain evaporates.

Eqs. (36) and (40) and putting in numerical values for the constants we can now write

$$9{\cdot}96 \times 10^{10}\, t = -\frac{1}{X}\int_{\theta_1}^{\theta_2} \frac{e^{0{\cdot}0343\,\theta}}{(\rho_\theta - \rho)^{1{\cdot}064}}\, d\theta \quad . \quad . \quad . \quad . \quad (41)$$

and from Eq. (39)

$$\rho = A - 4{\cdot}419 \times 10^{-7}\, \theta \quad . \quad . \quad . \quad . \quad (42)$$

where A is determined by the initial conditions of temperature and humidity and t is the time (in seconds) for the temperature to fall from θ_1 to θ_2 owing to evaporation of the raindrops. The shape of the temperature-time curve is thus determined by the integral on the right hand side of Eq. (41) but the time scale is determined by X. The shape of the humidity-time curve is related to the shape of the temperature-time curve. Thus the problem is now reduced to two parts : the evaluation of the integral on the right-hand side of Eq. (41) to determine the shape of the temperature-time and humidity-time curves and the evaluation of X to determine the time scale. We deal with these separately below. However, we must first justify the assumption that G is independent of θ.

15. Variation of velocity factor G with θ and with a

The velocity factor G is given by Eq. (7). Values were computed for several different drop sizes using the terminal velocities appropriate to $\theta = 288°\text{A}$ and

$\theta = 314°A$ and a pressure 1013·2 mb (Best 1950b). The value of G varied from 1·94 to 1·87 as the temperature increased from 288°A to 314°A for a drop radius 0·015 cm. For a drop radius 0·25 cm the corresponding figures were 18·5 and 17·2. The variation is so small that it was decided to treat G as independent of θ over the range 288°A to 314°A and to use the means of the values for these two temperatures. Values for several different drop sizes were computed in this way. G is thus a function only of a. We shall consider the evaluation of X below and for this purpose it is useful to know an explicit form of the relationship between G and a. Using the mean values of G for the two temperatures 288°A and 314°A, $\log_{10} G$ has been plotted against $\log_{10} a$ in Fig. 10. The resulting points approximate closely to a straight line the equation of which is

$$G = 61{\cdot}1\, a^{0{\cdot}85} \qquad (43)$$

16. Shape of the temperature-time and humidity-time curves

Having justified Eq. (41) we now return to a consideration of the shape of the temperature-time curve.

Let the initial values of f, ρ_θ and θ before the rain begins be denoted by f_0 (< 1), ρ_0 and θ_0 respectively. As evaporation from the rain continues the ambient temperature falls and the humidity rises. This process continues until the ambient vapour density ρ is equal to ρ_θ, the saturation vapour density at the air temperature then prevailing. Subsequently there will be no further change in ρ and θ. We can compute the depression of this steady-state temperature below the initial temperature as follows. From Eq. (42) we see that

$$\rho_\theta - \rho = \rho_\theta + 4{\cdot}419 \times 10^{-7}\,\theta - (f_0\,\rho_0 + 4{\cdot}419 \times 10^{-7}\,\theta_0) \qquad (44)$$

For convenience write

$$u = \rho_\theta + 4{\cdot}419 \times 10^{-7}\,\theta$$

$$v_0 = f_0\,\rho_0 + 4{\cdot}419 \times 10^{-7}\,\theta_0$$

Then u is a function only of θ and v_0 is completely determined by the initial conditions. Moreover, from Eq. (44),

$$\rho_\theta - \rho = u - v_0$$

The steady-state temperature θ_1 is reached when $\rho_\theta = \rho$, i.e., when $u = v_0$. By evaluating u for various values of θ we can obtain, by interpolation, the value of θ_1, which satisfies this condition for the specified initial values f_0 and θ_0. Values of $(\theta_0 - \theta_1)$ obtained in this way for nine sets of initial conditions are shown in Table XII. These values are independent of the rate of rainfall.

TABLE XII. Depression of steady-state temperature below initial temperature ($\theta_0 - \theta_1$) for various initial conditions

	θ_0 (°A)		
f_0	293	303	313
0·5	6·66	8·46	10·18
0·7	3·81	4·75	5·64
0·9	1·20	1·50	1·76

Returning now to Eq. (41) we can write

$$Y = 9{\cdot}96 \times 10^{10}\, t_\theta\, X = -\int_{\theta_0}^{\theta} \frac{e^{0{\cdot}0343\,\theta}}{(u - v_0)^{1{\cdot}064}}\, d\theta \qquad . \qquad . \qquad (45)$$

where t_θ is the time required for the temperature to fall from the initial value θ_0 to θ. The function Y is a direct measure of t_θ but the time scale can be determined only after X has been evaluated from a consideration of the number and size distribution of the raindrops. The integral on the right-hand side of Eq. (45) has been evaluated graphically for various values of θ, for three values of θ_0 and three values of f_0. These computations gave temperature-time curves, the time being expressed in terms of Y. Humidity-time curves were then obtained by means of the relation $f = 1 - (u - v_0)/\rho_\theta$. The results are shown in Figs. 12 and 13. It can be seen that, as would be expected on intuitive grounds, the excess of temperature above the steady-state value θ_1 diminishes exponentially and the deficit of relative humidity below 100 per cent decreases exponentially. The initial relative humidity has no effect on the temperature-time curve and the initial temperature has no effect on the humidity-time curve (within the range of these variables represented in Figs. 12 and 13). The initial temperature does, however, have a very slight effect on the temperature-time curve and the initial relative humidity has a slightly greater effect on the humidity-time curve though in each case it has not been considered worth while to draw more than one line on the figures.

The points depart from straight lines in Figs. 12 and 13 after the change in temperature or humidity has reached about 0·8 to 0·9 times its ultimate value. This may be due to the inevitable inaccuracies in the computations at high relative humidities

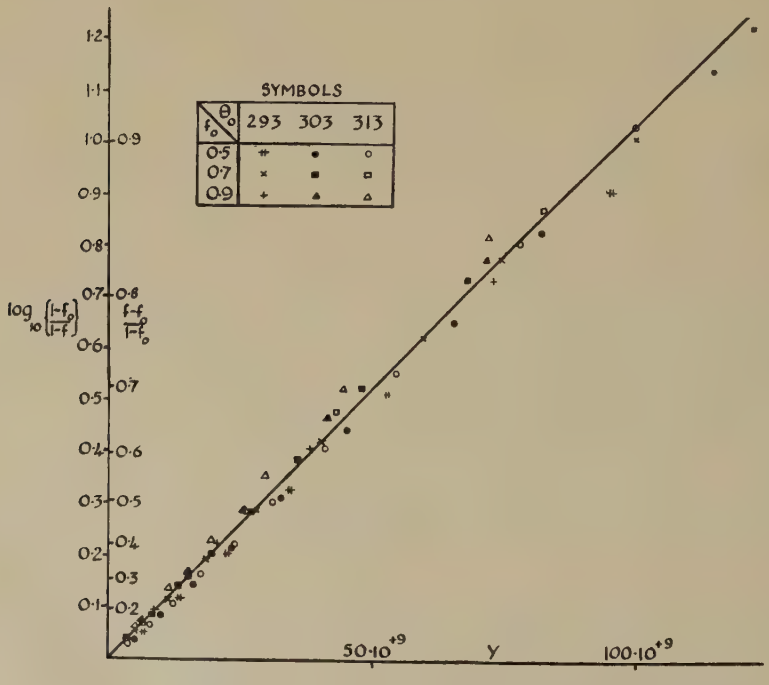


Figure 13. Variation of relative humidity with time as rain evaporates.

already mentioned though this seems rather unlikely. For practical purposes however the effect does not seem important.

Table XII shows the ultimate change in temperature for each set of initial conditions. Over the range of variables considered it appears that to a close approximation $(\theta_0 - \theta_1)$ is a linear function of θ_0 and, with less accuracy, a linear function of f_0. The equations to the lines in Figs. 12 and 13 are

$$\frac{\theta_0 - \theta_1}{\theta - \theta_1} = \exp(2{\cdot}74 \times 10^{-11}\, Y)$$

$$\frac{1 - f_0}{1 - f} = \exp(2{\cdot}39 \times 10^{-11}\, Y)$$

17. The time scale of temperature-time and humidity-time curves

Finally, we turn to the evaluation of X, defined by Eq. (38). Of the three factors in the integrand aGN the first needs no discussion and G has already been discussed in Section 15.

If at some fixed level above the surface the number of drops per unit volume of air with radius between a and $a + da$, $N\,da$, is constant with time then at any lower level $N\,da$ will vary with time as a result of evaporation below the fixed level. To introduce such considerations here, however, would lead to great complexity as it implies that N varies according to conditions above the surface and we shall therefore consider only the value of X when N is constant with time.

If the drop-size distribution is defined by Eq. (24) then we can easily show that (using also Eq. (43))

$$aGN = \frac{183{\cdot}3\ Wn}{4\pi}\left(\frac{2}{b}\right)^n a^{n-2{\cdot}15} \exp\left[-(2a/b)^n\right]$$

and from this it easily follows that

$$X = \frac{101{\cdot}6\ W}{\pi\, b^{1{\cdot}15}}\,\Gamma\left(1 - \frac{1{\cdot}15}{n}\right) \qquad (46)$$

The parameter W is not convenient for the present considerations and we shall now replace it by $I \times 10^{-6}/67\, b^{0{\cdot}73}$ from Section 11 (remembering that here W must be measured in g cm^{-3}). We still have the parameter b in the expression for X and since b is not normally measured and our aim is to interpret the temperature- and humidity-time curves in terms of familiar variables we shall use the experimental result $b = 0{\cdot}13\, I^{0{\cdot}232}$ (Best 1950a). With these substitutions Eq. (46) becomes

$$X = 22{\cdot}27 \times 10^{-6}\, I^{0{\cdot}564}\,\Gamma\left(1 - \frac{1{\cdot}15}{n}\right) \qquad (47)$$

where I is measured in mm hr^{-1}. Now the values of $\Gamma\left(1 - \frac{1{\cdot}15}{n}\right)$ for $n = 2$, 3, 4 and 5 are 2·08, 1·45, 1·28 and 1·20 respectively. We shall adopt a value of 1·6, corresponding approximately to $n = 2{\cdot}5$, this being about the mean value obtained elsewhere (Best 1950a). Then from Eq. (45) we have

$$Y = 3{\cdot}55 \times 10^6\, I^{0{\cdot}564}\, t_\theta \qquad (48)$$

The coefficient of t_θ in Eq. (48) has the following values :

I (mm hr^{-1})	1	5	10	25	50
$3{\cdot}55 \times 10^{-1} I^{0{\cdot}564}$	0·355	0·880	1·301	2·180	3·221

These figures determine the time scale of the curves in Figs. 12 and 13. For example, we see that if rain falls at 10 mm hr^{-1} and the air at the surface is initially at a temperature of 303°A and relative humidity 50 per cent the temperature will fall eventually to $(303 - 8{\cdot}46)$°A $= 294{\cdot}5$°A. The temperature will have reached $(303 - 8{\cdot}46 \times 0{\cdot}8)$ $= 298{\cdot}23$°A after $59 \times 10^9/1{\cdot}3 \times 10^7 = 4530$ sec and the relative humidity will have risen to $(50 + 50 \times 0{\cdot}8) = 90$ per cent after $67{\cdot}5 \times 10^9/1{\cdot}3 \times 10^7 = 5190$ sec.

In practice, of course, the rate of change of the surface temperature and humidity would be somewhat more rapid than indicated (even with no downdraught) if the value of I is measured at the onset of precipitation. As the rain continues the evaporation in the upper air diminishes and so the rate of precipitation at all lower levels increases. The time scale, however, varies inversely approximately as the square root only of I.

18. Comparison with Dolezel's work

Dolezel (1944) has considered the effect of falling rain on the temperature and humidity of the ambient air. He was concerned primarily with conditions which exist near a warm front and selected a particular set of circumstances for examination. His treatment was less general than that given here inasmuch as he considered only one air temperature, 10°C, (though he did deal with three different lapse rates of air temperature) and only one drop-size distribution. Dolezel's basic equations were similar to those employed here.

Despite various minor differences in the two treatments, values deduced from the present general treatment should be of the same order as those obtained by Dolezel for his special cases. From Fig. 6 of Dolezel's paper we select some results corresponding to $\theta_0 = 283$°A and $f_0 = 0{\cdot}5$. The ambient air was assumed to be isothermal. These results are given in Table XIII together with corresponding values obtained from the present treatment. The value of $(\theta_0 - \theta_1)$ for the present treatment was obtained by extrapolation from Table XII. The remaining figures were obtained by inserting values for I in Eq. (48) and then using Figs. 12 and 13.

TABLE XIII. Comparison of Dolezel's results with those from the present treatment for $\theta_0 = 283$°A and $f_0 = 0{\cdot}5$

			After 1 hr		After 2 hr	
	I (mm hr^{-1})	$\theta_0 - \theta_1$ (°C)	$\theta_0 - \theta$ (°C)	f	$\theta_0 - \theta$ (°C)	f
Dolezel	3·56	4·7	3·0	0·79	4·1	0·92
Figs. 12 and 13	3·56	4·9	2·5	0·73	3·7	0·87
Dolezel	1·78	4·7	1·7	0·65	2·8	0·76
Figs. 12 and 13	1·78	4·9	1·9	0·67	3·0	0·79

It is considered that the agreement between the two sets of figures is as good as could be expected.

19. Acknowledgment

Acknowledgment is made to the Director of the Meteorological Office for permission to submit this paper for publication.

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Appendix

The formulae used for calculating the various physical properties of air and water were as follows :

$$L = 597 - 0{\cdot}552\,t \text{ cal. g}^{-1}$$

$$D = 0{\cdot}219\,\frac{1013{\cdot}2}{P}\left[\frac{T}{273}\right]^{1{\cdot}853} \text{ cm}^2 \text{ sec}^{-1}$$

$$k = 5{\cdot}72\,10^{-5}\,(1 + 0{\cdot}00298\,t) \text{ cal cm}^{-1} \text{ sec}^{-1} \text{ °C}^{-1}$$

$$\mu = 1715\,10^{-7}\,\frac{273 + 117}{T + 117}\left[\frac{T}{273}\right]^{3/2} \text{ g cm}^{-1} \text{ sec}^{-1}$$

where t and T are temperatures in °C and °A respectively and P is barometric pressure in millibars. It may appear that the accuracy of the work does not justify a variation of L with temperature. However since it was necessary to allow for variation of some of the physical properties of air and water with temperature it seemed more consistent to treat them all in the same way.

The authorities for these formulae are :

L — Above 0°C values calculated from this formula agree with values quoted by Dorsey (*Properties of ordinary water substance*, New York (Reinhold 1940)). Below 0°C the formula is that assumed by Whipple (*Monthly Weather Review*, March 1927) and leads to calculated values of saturation vapour pressure in good agreement with those published by Washburn.

D — This formula is given by Dorsey (*loc. cit.*).

k — This is in agreement with the published measurements of Hercus and Sutherland (*Proc. Roy. Soc. A*, **145**, p. 599, 1934).

μ — See *Tables of Physical and Chemical Constants*, Kaye and Laby, 9th Edn., London (Longmans Green), p. 40.

The I.C.A.N. standard atmosphere is defined by a pressure 1013·2 mb and a temperature 15°C at the surface and a constant lapse rate of temperature up to 11 km of 6·5°C per km. The summer tropical atmosphere (see *Meteorological Glossary*, 3rd Edn., London (H.M.S.O.), p. 179) has the same surface pressure and temperature lapse rate as the I.C.A.N. atmosphere but has a surface temperature of 41°C.

551.577.11 : 551.574.1

Some characteristics of stratiform clouds over North Ireland in relation to their precipitation

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(Manuscript received 7 November 1951, in revised form 29 January 1952)

Summary

Analysis of stratiform-cloud data from routine aircraft ascents made at Aldergrove during 1942-4 and of ground-station records of precipitation reveals that *drizzle* fell predominantly from clouds warmer than − 5°C and thicker than 600 m, suggesting growth of drops by coalescence. Rain fell occasionally from clouds not extending above the − 5°C level but fell mainly from clouds with tops colder than − 12°C suggesting that ice crystals were mainly responsible for its release. The frequency and persistence of precipitation are analysed with respect to the temperature structure and the depth of the clouds. Some differences between the characteristics of precipitating and non-precipitating clouds are brought out.

1. Introduction

It is generally accepted that the Bergeron-Findeisen process, which requires the presence of ice crystals, is responsible for releasing much of the precipitation from stratiform clouds in temperate latitudes, but it is also possible that rain may be released by the collision and resultant coalescence of cloud droplets, by virtue of their random motion and differential rates of fall. In order to discover something of the conditions under which the two processes may release precipitation from layer clouds, a detailed analysis was made of the data obtained from aircraft ascents made thrice daily to a level of 400 mb, (the ' Thum ' ascents), during the period 1 May 1942 to 1 Dec. 1944, over Aldergrove (N. Ireland) and of the surface-weather reports from the same station. Since the aircraft ascents were made in a spiral not exceeding 10 mi in radius, it appears permissible to correlate the aircraft observations with the precipitation simultaneously reaching the ground at the airfield.

2. Analysis of aircraft and ground-station data

Aircraft reports of cloud type and amount, of the pressures and heights of cloud base, cloud top and inversions, and of temperature and humidity were examined together with ground-station hourly reports of precipitation and autographic rain-gauge records. The aircraft data were taken direct from the original reports (A.M. Form 2075).

The onset, continuity and type of precipitation were taken from the hourly reports in the *Large pocket registers*, as the autographic rain-gauge records were not sufficiently accurate and reliable, particularly when the precipitation was in the form of drizzle or snow. An example of the information which was extracted from the data is given in Table I. In selecting precipitating clouds for further study only those cases were chosen in which little doubt existed as to which single cloud layer was producing the precipitation; all cirriform cloud was ignored and also very thin layers of other high cloud if they were widely separated from much thicker lower layers. All occasions were discarded on which either showers or cumuliform clouds

(other than fracto-cumulus) were reported either by aircraft or ground observers within two hours of the ascent. The details from the 'THUM' aircraft reports were checked against those from reports of independent ascents to higher levels, 'PRATA,' on those occasions when the 'PRATA' flights were made at about the same time.

3. CHARACTERISTICS OF PRECIPITATING CLOUDS

The precipitating clouds were analysed with respect to the lowest temperature encountered by the aircraft inside them, the clouds producing drizzle at the ground being separated from those producing rain or snow. In practice, of course, the distinction between rain and drizzle is rather arbitrary, but the small number of occasions on which rain and drizzle were reported indicates that the proportion of border-line cases was probably small. Fig. 1 shows that in only 4 of the 99 well-substantiated cases of rain was the cloud-top temperature above 0°C, and in only 8 cases was it above — 5°C. Details of these 8 clouds are shown in Table I; three produced moderate and five only light rain, and in only one case was the precipitation at the ground continuous during the aircraft ascent. It will be seen that this only case of continuous moderate rain was associated with a warm, fairly deep layer.

The outstanding fact which emerges from a similar analysis of the 45 reported cases of drizzle is that, on the majority of occasions, it fell from clouds which were very unlikely to contain ice crystals. In 26 cases, the cloud-top temperature was above 0°C, and in 39 cases, above — 5°C. This indicates that the coalescence mechanism was predominantly responsible for the growth of drizzle drops in these clouds. There was a marked increase in the frequency of rain/snow when the cloud-top temperature

TABLE I. OCCASIONS WITH RAIN APPARENTLY FALLING FROM CLOUDS WARMER THAN — 5°C

Date	Ascent Time (GMT)	Surface report Time (GMT)	Weather	Cloud Type	Cloud Amount	Cloud Base Press. (mb)	Cloud Base Ht. (ft)	Cloud Base Temp (°C)	Cloud Top Press. (mb)	Cloud Top Ht. (ft)	Cloud Top Temp (°C)	Cloud thickness	Av. humidity between layers (per cent)	Remarks
.6.42	2000–	20	c r r	Ns	10/10	1006	300	16	740	8800	5	8500	100	4/10-6/10 Cs } Not
	2100	21	c r_0 r_0 m_0	As	10/10	680	11000	1	665	11600	0	600		2/10-3/10 Ci } reached
.9.42	1810–	18	c	St and	8/10	900	2700	4	740	7800	— 4	5100		9/10 Cc at 380 mb
	1910	19	c/i r_0	Fc										Cu reported in log-book at 17 hr but not later, probably Fc. Aircraft reports rain at 800 ft.
10.42	1300–	13	c r r m_0	St	10/10	990	600	11	875	3900	6	3300		5/10 Ac 470-450 mb
	1400	14	c/r r	Ac	7/10	600	13800	— 8	580	14700	— 10	900	81	7/10 Ci not reached
12.42	0812–	08	c	Sc	10/10	950	1500	8	830	5100	3	3600		5/10 Ci at 530 mb, top not reached. Data confirmed by Prata at 08 hr giving Ci top at 200 mb.
	0910	09	c i r_0											
6.43	1748–	17	c	Sc	5/10	980	900	15	880	3800	9	2900 }		1/10 Ac 610-440 mb
	1845	18	c r r m	Sc	9/10	880	3800	9	820	5800	6	2000 }		5/10 Ci not reached
		19	c/r r											
12.43	1724–	17	c m_0 ir_0	Ns	10/10	950	1800	4	800	6300	— 3	4500	94	
	1840	18	c m_0 ir_0	Ac	5/10	700	9800	— 8	690	10100	— 9	300		
9.7.44	0530–	05–06	c i r_0 m_0	Ns	10/10	940	2200	13	780	7300	5	5100	100	1/10 Ac 520-500 mb
	0648	07	c m_0 i r_0	Sc	10/10	720	9400	2	650	12100	— 4	2700		
5.10.44	0445–	05	c i r_0 m_0	Sc	10/10	880	4000	4	620	13300	— 5	9300		5/10 Cc 430-400 mb
	0605	06	c i r_0 m_0											

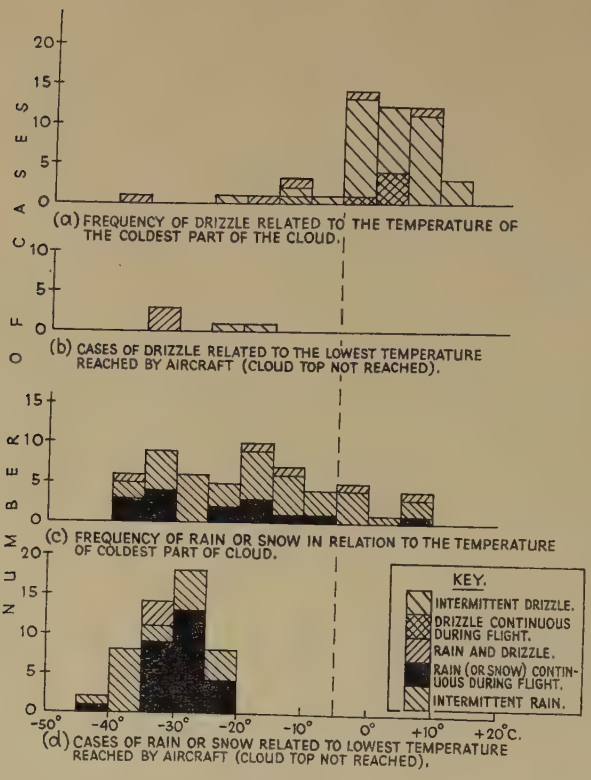
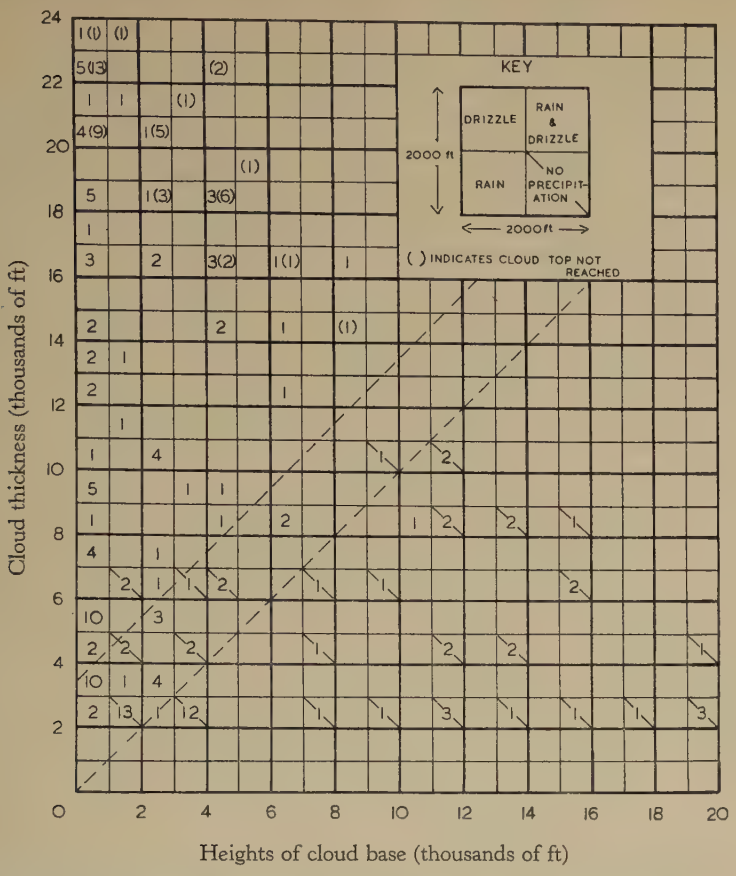

NUMBER OF CASES
(a) FREQUENCY OF DRIZZLE RELATED TO THE TEMPERATURE OF THE COLDEST PART OF THE CLOUD.
(b) CASES OF DRIZZLE RELATED TO THE LOWEST TEMPERATURE REACHED BY AIRCRAFT (CLOUD TOP NOT REACHED).
(c) FREQUENCY OF RAIN OR SNOW IN RELATION TO THE TEMPERATURE OF COLDEST PART OF CLOUD.
KEY.
INTERMITTENT DRIZZLE.
DRIZZLE CONTINUOUS DURING FLIGHT.
RAIN AND DRIZZLE.
RAIN (OR SNOW) CONTINUOUS DURING FLIGHT.
INTERMITTENT RAIN.
-50° -40° -30° -20° -10° 0° +10° +20°C.
(d) CASES OF RAIN OR SNOW RELATED TO LOWEST TEMPERATURE REACHED BY AIRCRAFT (CLOUD TOP NOT REACHED).

Figure 1. Frequency distribution of rain and drizzle with respect to the lowest temperature recorded in the cloud.

fell below — 12°C, suggesting that only below this temperature does the concentration of ice-forming nuclei, on the average, become significant for precipitation release. Clouds which produced snow at the ground were, in every case, colder than — 12°C at their upper boundaries. Fig. 1 shows that precipitation, especially continuous rain or snow, is more likely the thicker the cloud; but as the thicker clouds were generally associated with lower temperatures in their upper parts this fact may be partly attributed to the continuous increase in the average concentration of ice nuclei with falling temperature (Findeisen and Schulz 1944, Palmer 1949). Rain/snow fell only from clouds of thickness $>$ 3,300 ft ($\simeq$ 1,000 m) and continuous rain only from those thicker than 7,500 ft (2,250 m), while *no* precipitation reached the ground from clouds of depth $<$ 1,500 ft ($\simeq$ 450 m). All the precipitating clouds had bases warmer than — 5°C.

4. Characteristics of non-precipitating clouds

In an attempt to discover other necessary conditions for the release of precipitation from layer clouds, an analysis was made of a representative number of occasions during 1942-4 on which there were at least seven-tenths of low or medium cloud with no precipitation appearing at the ground within at least four hours of the aircraft ascent. On the majority, 186 out of 252, of such occasions the cloud thickness was less than 2,000 ft; this, together with the fact that there were only two cases of drizzle falling from clouds in this category suggests that 2,000 ft may be taken as the lower



igure 2. The effect of cloud thickness and height of cloud base in distinguishing between precipitating nd non-precipitating clouds. The number of cases in each interval of 2000 ft is given in the square appropriate to the type of precipitation.

miting thickness of the precipitating clouds whose base temperatures varied between − 5°C and + 16°C. A distinction between the remainder of the non-precipitating louds and those which produced rain, snow or drizzle is brought out in Fig. 2, showing he thickness and the height of the base of each cloud layer. In the majority of cases he thickness of the clouds failing to produce precipitation at the ground was considerably ess than their height above ground. Fig. 2 shows the following criteria distinguish he precipitating from the non-precipitating clouds. Clouds of thickness less than he height of the cloud base, (with one exception), produced no precipitation, while louds of thickness greater than the height of the base plus 3,500 ft, (with three excep-ions), precipitated. Of the non-precipitating clouds which probably contained ice rystals (top temperature < -5°C), all but three had base temperatures lower than °C.

5. Discussion

The observations show that there is a high probability of rain or snow falling rom thick clouds with cold tops, also if their base temperatures are in the neighbourhood f 0°C or above. Clouds of thickness less than 7,500 ft, cloud-top temperature above

— 12°C and with bases appreciably colder than 0°C rarely gave rain or snow. Clearly, development of these types of precipitation will be favoured in clouds whose tops are cold enough to ensure activation of sufficient numbers of ice nuclei and of sufficient depth for the crystals to grow large enough to survive evaporation in the layer between the cloud base and the ground. One then would expect the ratio of the cloud thickness to the height of the cloud base to be a relevant factor in deciding whether precipitation will reach the ground. If the cloud has a supercooled region of considerable depth (i.e., cloud-base temperature not appreciably lower than 0°C), the growth of precipitation elements will be aided in two ways; the ice crystals increasing in mass by colliding with the supercooled droplets and the supercooled water helping to cement together individual crystals to form snowflakes. Quantitative treatments of the precipitation processes in layer clouds will be given in future papers.

Acknowledgments

We are indebted to the Director of the Meteorological Office and to Dr. J. Glasspoole for access to the rainfall records and the station registers. One of us (B.P.H.) wishes to acknowledge grants from the Ministry of Education and the Norwegian Government.

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551.506.2(41/2) : 551.515.1 : 551.577.51

A study of the rainfall of two synoptic situations

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(Manuscript received 15 January 1952, in revised form 25 February 1952)

Summary

The rainfall resulting from the movement of the small depression of 14 March 1949 across the British Isles is analysed in detail by means of the autographic records and rainfall observations maintained for climatological purposes. The most striking features of the rainfall distribution are the result of orographic lifting and are in reasonable accord with theoretical expectation. The main effect of the hills is to intensify the rainfall in the existing rain area, but the duration is also increased in the higher areas. Other features of the rainfall distribution are discussed in relation to Sutcliffe's theory of development.

A similar analysis is also made of the rainfall from the warm front of 14 February 1950.

1. Introduction

A great wealth of information is recorded regarding the rainfall of each day over the British Isles. This consists of daily measurements of rainfall and of the records of recording raingauges at an extensive network of places over the country. The information is of a much more detailed character than is used in synoptic practice and, because of the labour of collecting and analysing the material, it is doubtful if it can be used even in research when a large number of occasions are to be studied. The present investigation makes use of most of the available information regarding the rainfall on two occasions. Its purpose is to establish with some certainty the rainfall distribution in the selected cases and to ascertain whether it can be explained by physical and dynamical principles.

The two cases selected are chosen to be typical of steady warm-front rain over England, although the choice is also guided by the need to be able to separate the rainfall of the disturbance from the rain of preceding and following rain areas in the records of gauges read at 24-hour intervals. The two situations studied are:

(a) that of 14 March 1949, when a small depression moved east-southeastward over England and Wales.

(b) that of 14-15 February 1950, when an active warm front moved northeastward over the whole of the British Isles.

2. The depression of 14 March 1949

(a) *The synoptic situation*

At 00 GMT on 14 March 1949 a deep depression over Norway was moving slowly east and an anticyclone was almost stationary near the Azores. Between these two systems a belt of strong thermal gradient extended from west-northwest to east-southeast over the British Isles. A small depression was situated about 600 mi west of Scotland, and this depression subsequently moved quickly east-southeast across northern Ireland and central England, closely following the direction of the thickness lines and causing little distortion in them. The synoptic situation on the afternoon of 14 March 1949 is illustrated in Figs. 1 (a) and (b). The depression was analysed as

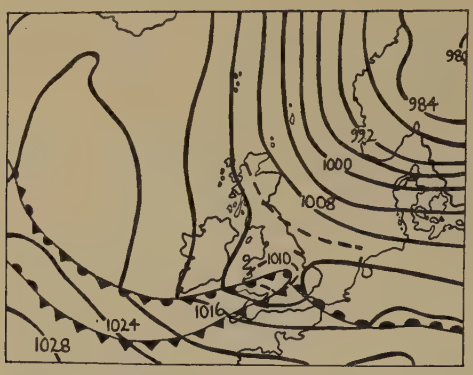


Figure 1 (a). Sea-level isobars 15 GMT 14 March 1949 (unit = mb).

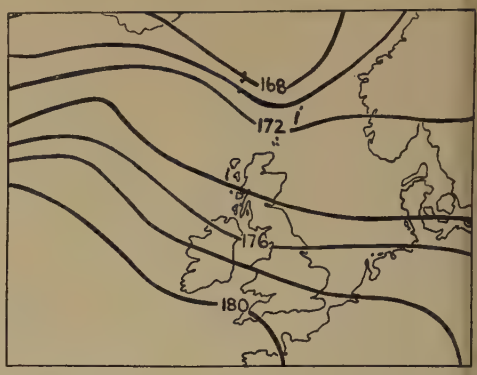


Figure 1 (b). Thickness chart 1,000-500 mb, 15 GMT 14 March 1949 (unit = 100 ft).

possessing a warm front, cold front and 'trailing occlusion.' Some evidence can be found for the passage of these fronts across England, but with the exception of some sections of the occlusion, the fronts were poorly defined – the cold front is particularly difficult to place. However, the centre of the depression can be followed without difficulty, moving along a track from County Mayo, to near Dublin and thence to Felixstowe, at about 40 kt. It was accompanied by an extensive area of rain.

(*b*) *The rainfall distribution in the moving rain area*

The rainfall distribution within the rain area has been reconstructed from the records of some 50 autographic raingauges maintained at official meteorological stations in England, Wales and Northern Ireland. (Records from privately maintained autographic gauges have not been used for this purpose, but a few such records have been used to study the time sequence of rain in the hilly areas of Wales and Devon). Approximate plots of rate of rainfall against time have been constructed for each station.

These diagrams clearly show that a broad similarity exists in the time distribution of rainfall at stations similarly situated with respect to the track of the depression. A composite chart was therefore constructed in which rate of rainfall is plotted for each station as a function of time along a line placed at a distance from the x axis corresponding to the distance of the station from the track of the depression. On this diagram mean rates of rainfall over hourly periods were plotted and mean values were formed for each area of the diagram. In this way a composite picture of the rate of rainfall in the depression can be built up from which the differences between individual records due to orographic influence, site or exposure are eliminated. Such a picture is presented in Fig. 2; small adjustments have been made to bring the time of occurrence of cold front or occlusion rain into coincidence, but general agreement between different stations regarding the limits of the rain area is good. In this form of diagram it is not possible to show adequately the rainfall in the right-hand rear sector where short periods of intense rainfall (reaching a rate equivalent to 10 mm hr^{-1} over periods of 10 min with the passage of the occlusion) occurred at some stations but not at others. The cold front also produced sporadic rain but of a less intense nature.

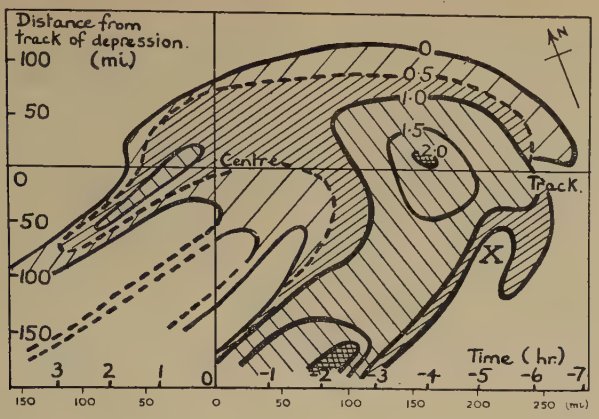


Figure 2. Distribution of raté of rainfall in depression of 14 March 1949 in relation to centre and track. (Isopleths at intervals of 0·5 mm hr^{-1}).

The main features of the rain area as analysed from the records of the autographic gauges are :

(i) A centre of maximum intensity of about 2 mm hr^{-1} on the track of the depression and about 200 mi ahead of the centre, with a secondary maximum of about 1 mm hr^{-1} just behind the centre.

(ii) A broad area of rainfall exceeding 0·5 mm hr^{-1} extending about 100 mi to the left of the track of the centre and reaching about 240 mi ahead of the depression and 50 mi behind it.

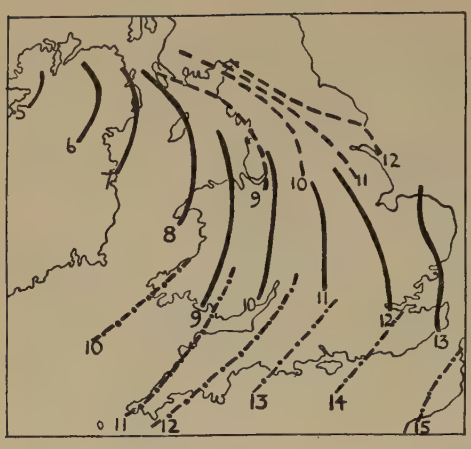
(iii) An area of fairly uniform rainfall exceeding 1 mm hr^{-1} in the forward right-hand sector; this was not associated at all closely with the surface warm front.

(iv) A series of belts of sporadic and rather heavy rain extending rearward and to the right of the track from the central region of the depression.

The onset of the measurable rain appears to have been abrupt at most stations – at no station would the curves be better described by a gradually increasing rate of rainfall. Nevertheless, it is not possible to identify a single moving line representing the onset of rain. As indicated in Fig. 2 the rain area appears to have possessed three distinct boundaries which advanced across the country in echelon. The isochrones of the position of these three boundaries are shown in Fig. 3. At some stations (e.g., Chivenor*) the passage of two of the boundaries can be identified with a cessation of rain between; at others (e.g., Croydon) an increase of the rate of rainfall took place as the station was passed by the extension of a boundary line into the rain area. It may be significant that the southern limit of the two northern boundaries occurred near a break in the high ground along the western seaboard; the more northerly at the gap between Pennines and Welsh Mountains, and the more southerly at the gap between the Welsh mountains and the hills of Devon.

The broad belt of rain in the right-forward sector appears to have maintained a reasonably constant position with respect to the depression as it moved across the country. The warm front itself did not do this. Thus we find that most of the rain fell behind the warm front at Chivenor and Bristol, but ahead of the warm front at Croydon and Lympne. The subsidiary maximum of rainfall at the southern end of

* The locations of some of the places referred to in the text are shown in Figure 4.



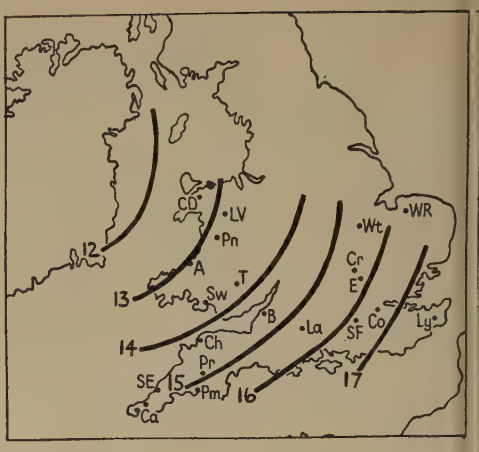


Figure 3. Isochrones of front edges of rain area on 14 March 1949.

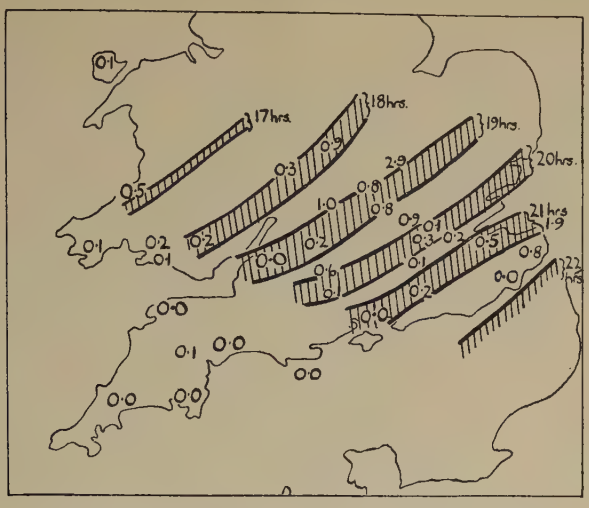
Figure 4. Isochrones of rear edge of rain area on 14 March 1949. Letters indicate the position of places mentioned in the text according to the following key : A = Aberporth; B = Bristol; Ca = Camborne; Ch = Chivenor; Cr = Cranfield; Co = Croydon; CD = Cym Dyli; E = East Hill; LV = Lake Vyrnwy; La = Larkhill; Ly = Lympne; Pm = Plymouth; Pn = Plynlimon; Pr = Princetown; SF = South Farnborough; SE = St. Eval; Sw = Swansea; T = Taf Fechan; WR = West Raynham; Wt = Wittering.

this belt of rain is probably not significant. The only stations in this region were St. Eval and Plymouth, and they may both have been subject to orographic increase of the rain. The rear edge of this rain belt is quite clearly defined by the autographic records although slight rain or drizzle continued with less than measurable intensity after the indicated rain area had passed. Isochrones of the end of continuous measurable rain are given in Fig. 4.

In the right-rear sector of the depression most stations experienced one or more short periods of rain of moderate or heavy intensity. The last of these can be defined easily; this is analysed as associated with the trailing occlusion which moved south in the rear of the depression. Isochrones of the position of this rain belt are given in Fig. 5. Only a band from S. Wales to Sussex and Kent was affected. To the southwest of this no measurable precipitation fell except possibly over Dartmoor, and to the northeast no well-defined separation occurred between the rainfall from the occlusion and that from the main rain area. Amounts of rain from the occlusion varied irregularly (see Fig. 5) but most stations had two bursts of moderate or heavy rain about 15 min apart.

It is uncertain from the autographic records whether the other short bursts of rain behind the main rain belt were arranged along linear belts of a frontal type or not. There is some suggestion of two such belts as indicated in Fig. 2, but if they existed many stations escaped rain from one or other of them. The original cold front of the system was so diffuse that it cannot be placed with any certainty, although it must have been present in this sector of the depression if it still existed at all.

The mean value of the rate of rainfall has been evaluated over all stations for a period of 11 hr during which the depression passed. The mean value obtained is 0·67 mm hr^{-1} and the standard deviation, 0·57 mm hr^{-1}. A calculation was also made of the departure of the rainfall from the smoothed average value appropriate



igure 5. Isochrones of occlusion-rain area on 14 March 1949. Amount of rain from occlusion in mm.

o the particular sector of the depression as presented in Fig. 2. The root mean square leparture is 0·39 mm hr^{-1}. A comparison of this figure with the departure from he overall mean shows that a large part of the variation of rainfall is accounted for by the structure of the rain area shown in Fig. 2. Noteworthy positive departures f the rates of rainfall from the values appropriate to the distribution in Fig. 2 occur t some west-coast stations and in East Anglia. The positive excesses at Aberporth + 5·6 mm) and Chivenor (+ 3·7 mm) might be expected from the onshore wind nd lifting by the hills only a short distance inland. The positive excesses at West Raynham (+ 3·2 mm) and Wittering (+ 3·4 mm) are less easy to explain. Negative leparture are found at stations east of the Welsh mountains, but also over Wiltshire nd the Thames valley, reaching — 3·3 mm at South Farnborough. These differences re presumably due to orographic effects and there is no clear sign that they operated ifferently in different sectors of the depression. Most of the places with an orographic ncrease had rainfall for much of the same period as stations with amounts nearer to he average, but the intensity appears to have been increased. On the other hand, at Princetown, 1,360 ft above sea level on Dartmoor, the rainfall commenced five hours arlier and was more continuous in the rear sector of the depression than at low level tations in the same area. However, only about 20 per cent of the rainfall fell at Princetown before rain commenced at neighbouring low-level stations.

(*c*) *The total rainfall from the depression*

A chart of the rainfall distribution resulting from the depression has been drawn n the basis of all 24-hour rainfall observations available in the Climatology Branch of he Meteorological Office. This provides a network of observations at about 10 mi ntervals over most of the country except the less populated hilly areas of Wales, Pennines, Devon and Cornwall. The resulting chart is shown in Fig. 6. Reports are vailable only to 0·1 in., but isopleths have been drawn on the basis of these figures at ntervals of 5 mm (0·2 in. approx.). In order to include all the rain from the depression

Figure 6. Distribution of rainfall from depression of 14 March 1949.

the isohyets are based on the rainfall from 09 GMT 14 March to 09 GMT 15 March over most of England, but from 09 GMT 13 March to 09 GMT 15 March over Northern Ireland, Wales and NW. England. Little rainfall is included other than that associated with the depression.

The most striking features of this detailed rainfall map are the maxima of rainfall over Wales and over Dartmoor and Exmoor, where orographic effects are pronounced but this should not obscure the general distribution of precipitation resulting from the depression which includes a band of high rainfall along the track of the centre of the depression from N. Wales to Essex. This band is clearly defined over eastern England where orographic effects are small. Rainfall falls off fairly rapidly to the north and less rapidly to the south of this band. The effects of various orographic features can be identified by maxima in the rainfall distribution, e.g., the Mendip Hills, the South Downs and the higher ground of East Anglia. The location of the rainfall minima, presumably rain shadows, is more surprising. The small dry area east of Dartmoor is to be expected, and the low rainfall round Gloucester and Hereford but the dry areas in central Hampshire and the Vale of the White Horse are more difficult to explain.

(*d*) *Distribution of rain as observed by radar at East Hill*

A comprehensive set of photographs is available for this occasion from the meteorological radar at East Hill (near Dunstable, Bedfordshire). These confirm in some detail the horizontal structure of the rain area of the depression as deduced from

ne autographic rainfall records. Photographs of the P.P.I. tube from 1100 to 1130 MT show the boundary of the rain area approaching from the west with an alignment rom north-northwest to east-southeast corresponding closely to the appropriate section f Fig. 2. East Hill remained in the rain area until 1530 GMT, but from 12 to 13 GMT ne radar photographs show that the rainfall fell off southward rather sharply at a istance of 25 to 30 mi corresponding to the section X of Fig. 2. The echo again xtended further southward from 1310 GMT until 14 GMT and this is presumably an ndication of the extensive belt of rain to the south.

A clearance of the main precipitation echo is clearly shown as moving over East Hill from the northwest at 1530 GMT. This agrees closely with the rear edge of the nain rain belt as deduced from the autographic records both in the time of clearance nd alignment of the edge of the rain area. Prior to this clearance, the radar photographs rom East Hill show a more patchy structure than before, with bands of relatively ntense echo and intermediate weaker bands aligned parallel to the rear edge of the ain area. Subsequent photographs of the P.P.I. tube up to 1630 GMT show a number of mall echoes scattered over the area but with no tendency to form into bands, except or one rather more extensive area of rain which appears to have formed over East Hill about 20 mi behind the main rain area and moved southeast behind it. This cho developed a definite alignment parallel to the preceding rain edge.

(*e*) *Rainfall of* 14 *March in relation to dynamical theory*

Since rainfall, not due to vertical instability or orographic lifting, is determined primarily by the large-scale upward motion of saturated air, it is possible to estimate he amount of rainfall if the vertical motion can be assessed. There are several ways n which this may be attempted; kinematically, by measuring the horizontal divergence nd using the equation of continuity; thermodynamically, by following a conservative roperty such as wet-bulb potential temperature; or dynamically, by inferring divergence rom change of vorticity. All these methods are laborious and since, in the nature of the problem, nothing more than a broad estimate can be expected, we have chosen o compute the values assuming a simple dynamical model and using a modification of Sutcliffe's (1947) theory. This theory rests on a number of approximations, but vill serve as an indication of the relationship between rainfall and dynamical development.

Sutcliffe gives an expression for the cyclonic development in terms of the distribution of the height of the 1,000 mb surface and the thickness distribution of an appropriate isobaric layer of the troposphere. On certain further assumptions of vhich the most important is the absence of vertical motion at the tropopause it is possible to estimate the rate of rainfall corresponding to cyclonic development of a given intensity (Sawyer (unpublished)). Sutcliffe's treatment assumes neutral vertical stability, but further work by Sumner (1950) and Bushby (1951) has shown that if the stable stratification of the atmosphere is taken into account the vertical velocities will be considerably reduced. The numerical solution of the problem by F. H. Bushby n selected cases suggests that reduction to a quarter is normal.

In order to make an approximate assessment of the amount of rainfall to be expected if these theoretical ideas are valid, Sutcliffe's expression for the development vas evaluated for 6-hour intervals during the passage of the depression. The expression used was

$$-V' \partial/\partial s (\zeta' + 2\zeta_0)$$

where V' is the thermal wind velocity, ζ' is the vorticity of the thermal wind field, ζ_0 is the vorticity of the geostrophic wind field at a low level (1,000 mb) and $\partial/\partial s$ denotes differentiation in the direction of the thermal wind. (The expression as evaluated omits the small term $-V' \partial l/\partial s$ included in the expression in Sutcliffe's paper). This expression was evaluated for the layer 1,000-500 mb, and in order to estimate the total effect of upward motion in producing rainfall the positive contributions have been added together and the rainfall which would have been expected is indicated in Fig. 7. For this purpose it is assumed that if the expression $-V' \partial/\partial s (\zeta' + 2\zeta_0)$ had been evaluated up to the tropopause (200 mb approx.) it would have been increased in proportion to the depth of the layer, i.e., approximately by a factor of two. On the other hand it is assumed that the damping effect of stability would have been as indicated above (i.e., to reduce the vertical velocity to a quarter of that in an atmosphere of neutral stability). It will be seen that the direct application of the development principles leads to the expectation of a broad band of precipitation across the British Isles of about the correct intensity. The axis of heaviest rainfall in fact occurred about 100 mi south of the position which would have been expected. It is unlikely that the appropriate vorticity gradients could have been evaluated sufficiently accurately on the 1,000 mb and thickness charts to place the maximum of development with an error less than 100 mi, but it is interesting to note that the numerical solution by Bushby of the equation representing the damping effect of vertical stability does suggest that the maximum of vertical velocity should have occurred slightly to the south of the maximum of Sutcliffe's expression for development.

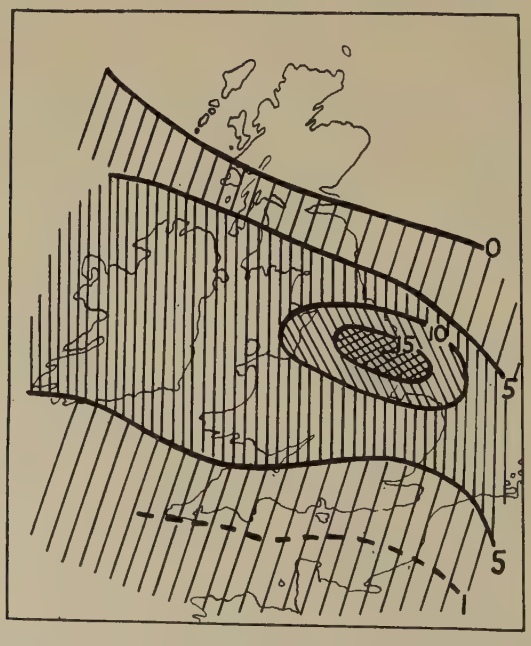


Figure 7. Rainfall equivalent of the integrated development terms, 14 March 1949 (unit mm).

(*f*) *Orographic influences on the rainfall*

The autographic records from Aberporth, Swansea and Chivenor show that, lthough these stations received considerably heavier rainfall than places inland, he duration of the significant rainfall was much the same. This is also true at higher-evel stations on Snowdon (Cym Dyli) and at Taf Fechan (Pontsticill) in South Wales. The only exception is that rain commenced earlier at Princetown than at low-level tations in Devon. These facts suggest that the effect of lifting by the hills further nland has been principally to increase the intensity of the rain in the dynamically roduced rain areas, rather than to produce additional orographic rainfall within the ir masses themselves. Douglas and Glasspoole (1947) have pointed out the need or a deep damp layer for heavy orographic rainfall to result. Such conditions certainly xist in the rain areas of the depression, but it may be that elsewhere the orographically roduced cloud is often too thin, or physically too stable, to cause effective precipitation. Within the pre-existing rain areas we can expect that any additional droplets produced by orographic lifting will be swept up by the falling rain and so increase the rainfall. If ain is not already falling, however, this may not necessarily occur.

An estimate may be made of the additional orographic rainfall which would be xpected if all additional condensation during the passage of the rain area reached he ground as rain. We follow the method adopted by Douglas and Glasspoole (*loc. it.*). It is reasonable to assume that the motion at some level is undisturbed by the opography, that the air near the ground follows the outline of the hills and that the ir at intermediate levels is lifted in proportion. If we further assume that the same mount of water is carried as cloud and raindrops throughout, it is a simple matter o compute the amount of water which must fall out of a given vertical column during ts ascent of a windward slope. This calculation has been made for a lifting of 1,000 nd 2,000 ft, an air column saturated and with a lapse rate following the saturated diabat for a wet-bulb potential temperature of 50°F (this is close to the actual conditions n the rain area on 14 March); various assumptions have been tried regarding the eight of the undisturbed level. The results are given in Table I.

TABLE I. DEPTH OF WATER THAT WOULD BE PRECIPITATED FROM AN AIR COLUMN SATURATED ALONG THE 50°F SATURATED ADIABAT WHEN LIFTED OVER A RIDGE OF HILLS

	Undisturbed level assumed to be :		
Height of hills (ft)	850 mb	700 mb	500 mb
1,000	0·05 cm	0·08 cm	0·12 cm
2,000	0·08 cm	0·15 cm	0·25 cm

The amount of orographic rainfall which falls at individual places depends on he slope of the barrier or hills and on the area over which the rainfall is spread. Over Exmoor on Fig. 6 the area of marked orographic rainfall probably only extends 25 mi rom east to west. We treat Exmoor as a barrier 1,000 ft high and assume that lifting extends to 500 mb. The wind at 800 mb during the rain period was approximately west by north and 40 kt and the rain area took about 5 hr to pass (at Chivenor), so hat 0·12 cm of water from an air mass 200 mi long was deposited over an area 25 mi ong – the total orographic fall should therefore have been about 0·96 cm. This

appears to be of the correct order as the maximum fall observed was 20 mm of which about 15 mm can be regarded as an orographic increment. Over the Dartmoor-Bodmin Moor area the rain was spread over about 35 mi depth and the duration may have been slightly less, but as the barrier is probably rather higher the maximum rainfall was about the same. The amounts of orographic rain in S. Wales can be broadly explained in the same way. The maximum of about 40 mm near Plynlimon in central Wales of which about 30 mm was probably due to orographic lifting requires a barrier of 2,000 ft and the rainfall deposited on an area of only 20 mi depth – not an unreasonable expectation on this section of the Welsh coast.

It is not possible to discuss the orographic rainfall in North Wales where the winds were irregular and variable, but it is noteworthy that very little orographic increase is observed on the South Pennines where low-level winds were light during the rain period.

Poulter (1936) has demonstrated that the orographic effect is less on cold-front rain than on warm-front rain. In view of the small contribution of the cold front and occlusion to the total rainfall at low-level stations, it is reasonable to omit the orographic increment to the rain in the cold sector in making the estimates of orographic rainfall given in the preceding paragraphs. Although this assumption has sufficed to obtain crude estimates of the rainfall in the hill districts (probably correct within 50 per cent), nevertheless the rain in the cold sector of the depression was very much greater at the high-level stations of Princetown and Taf Fechan than at adjacent low-level stations. At Princetown the cold-sector rain provided about 20 per cent of the total, at Taf Fechan rather less.

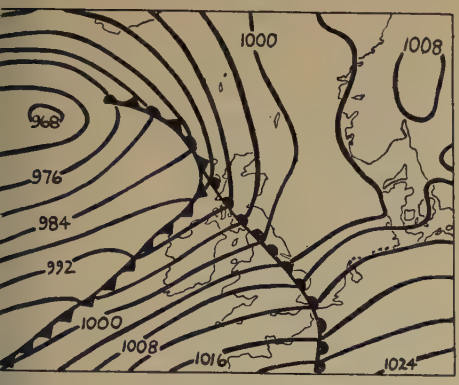
It is not the purpose of this paper to analyse in detail the pattern of orographic rainfall. This would require a detailed study of the local topography. However, it is clear that the broader features of the rainfall over the hilly districts can be explained on the simple hypothesis of the lifting of the saturated air of the rain area as it crosses the hills.

The distribution and intensity of the 'rain shadows' in the lee of the high ground is less easily explained. It has been indicated above that a barrier 1,000 ft high would be expected to have condensed from the air an additional depth of about 1 mm of water. This represents about an hour's rainfall on level ground as measured in most parts of the rain area ($\frac{1}{2}$ hr rain near the centre) and we might therefore have expected a completely dry area of 30 to 40 mi east of the Welsh mountains and Dartmoor or an area of reduced rainfall of much greater extent. In fact no such substantial reduction of rainfall can be noted. There are small dry areas east of Dartmoor and between Monmouth and Hereford but the relatively dry areas extending across Salisbury Plain from Bath to Southampton and Guildford and that between Swindon and Oxford are not related to the main orographic barriers in any obvious way.

3. The warm-front rain of 14 February 1950

(*a*) *The synoptic situation*

At 12 GMT on 14 February 1950, a deep depression was centred about 60 mi SW. of Iceland and was moving northeastward. Its warm front extended from northwest to southeast across the southwest approaches to the British Isles. The warm front moved northeastward across the country with only small irregularities and by 1200 GMT 15 February, it had reached the central North Sea. The warm front was



igure 8 (a). Sea-level isobars 03 GMT 15 February 1950 (unit = mb).

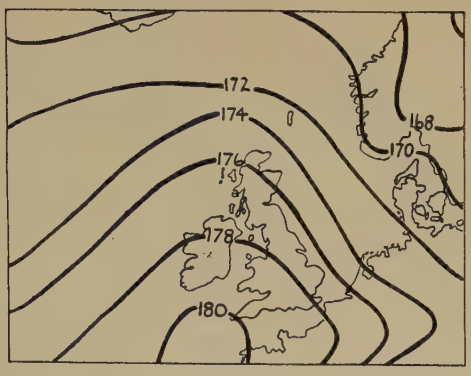


Figure 8 (b). Thickness chart 1,000-500 mb, 03G MT 15 February 1950 (unit = 100 ft).

receded by the well-defined rain belt which was selected for study. The cold front f the system came southward only as far as the Scottish Border before returning as warm front into a following wave depression, but the warm sector was in process f rapid occlusion as it crossed Scotland. (The synoptic situation is illustrated in Figs. (a) and (b)). The main depression was associated with a strong twisting of the 000-500 mb thickness lines and the warm sector was accompanied by a well-defined hermal ridge. A strong thermal contrast existed across the warm front; this amounted o over 20°F at the 700 mb level.

(*b*) *The rainfall distribution in the moving rain area*

The records from 66 autographic raingauges at stations in the British Isles have been examined and the rate of rainfall plotted as a function of time during the passage f the frontal system. The passage of the belt of warm-front rain is clearly shown by all the records in England and Wales and most of the rain occurred in advance f the warm front. Except at high-level stations there were only small amounts of ain in the warm sector and at most stations the amounts were insufficient to be ecorded. At high-level stations in Wales and on Dartmoor rainfall continued rregularly in the warm sector with an intensity of about 1 mm hr^{-1} (rather less on Dartmoor). Over Scotland the rainfall distribution was more complex because of he occlusion process which was going on rapidly and also on account of the strong rographic influences.

At most stations measurable warm-front rain lasted about four hours. The ntensity increased irregularly to a maximum one to two hours before the passage f the front as analysed from the surface charts, and measurable rain ceased in most ases a little before the arrival of the front. The intensity of the rain varied considerably rom place to place, presumably as a result of orographic influences. However, the lot of rates of rainfall in position relative to the front (as in Fig. 1 for the depression f 14 March 1949) and the calculation of average values over all of the stations in arious sectors show that there was little systematic variation in intensity along the ength of the front over England and Wales. Stations near the west coast of England nd Wales had a maximum intensity of about 2 mm hr^{-1} whereas some stations in astern England did not reach 1 mm hr^{-1}. There is also an indication that the duration

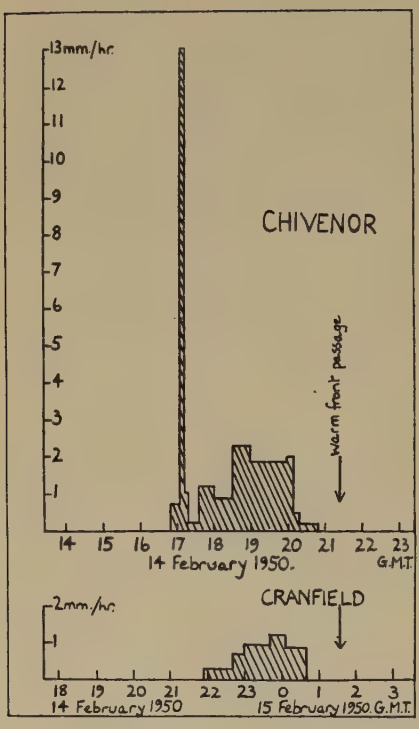


Figure 9. Rate of rainfall at Chivenor and Cranfield, 14 to 15 February 1950.

Figure 10. Distribution of rainfall from warm front t of 14 February 1950. Period 09 GMT 14 February to 09 GMT 15 February 1950.

of the warm-front rain was less at the stations with lighter rainfall, suggesting that situations relative to the front which gave only light rain on the west coast gave none at all at some eastern stations. We may compare the plots of rate of rainfall against time at Chivenor, Devon on the west coast with Cranfield, Bedfordshire in eastern England (Fig. 9).

At stations in southwest Scotland the distribution of rainfall with respect to the warm front was similar to that in England but the rainfall was much heavier, but at Renfrew and Eskdalemuir heavy rain continued at a rate of 3 to 4 mm hr^{-1} in most of the warm sector. Further to the north the rain appears to have been less intense, and on the east coast at Dyce, Lossiemouth and Wick amounts were similar to those in England but the rain area was displaced ahead of the warm front as analysed on the surface charts by a period of about two hours.

The average rate of rainfall for hours centred 0·5, 1·5, 2·5 etc., hours ahead of the frontal passage have been calculated over all stations in England and Wales and they are as follows :

Hours before warm-front passage	0·5	1·5	2·5	3·5	4·5	5·5	6·5
Rainfall (mm hr^{-1})	0·32	0·89	0·94	0·60	0·25	0·01	0

The average rate of rainfall for all stations in England and Wales over the 6 hr preceding the frontal passage is 0·50 mm hr^{-1} and the standard deviation of the hourly

rates from this mean is 0·51 mm hr^{-1}. The standard deviation from the average rate appropriate to the distance from the warm front as given in the preceding table has also been calculated. This is 0·34 mm hr^{-1} showing that the variation of intensity with the approach of the front does account for a substantial part of the variation of rate of rainfall at individual stations.

(*c*) *The total rainfall of* 14 *February*

Isohyets for the period 09 GMT 14 February 1950 to 09 GMT 15 February 1950 are given in Fig. 10 based on all available raingauge readings. It is seen that about 5 mm rainfall was received in places on the western coasts of England and Wales and in northern Ireland. Rather less than 5 mm fell in eastern and central districts of England and some districts of northeast Scotland.

Pronounced orographic maxima of rainfall are found over the Welsh mountains, Dartmoor, the Lake District and the mountains of west Scotland. The orographic effect is less well marked over the Pennines, possibly because the air had only recently crossed the Welsh mountains. Increase of rainfall may also be noted over the Mendip Hills and the South and North Downs. The most striking maximum of rainfall, however, is that centred a little southeast of Greenock. As well as including the high ground in that area this heavy rainfall also fell at stations on the banks of the Firth of Clyde. The autographic records from Renfrew and Eskdalemuir show that much of this rain fell in the warm sector.

(*d*) *Radar observations of rainfall distribution*

The East Hill meteorological radar station did not operate during the passage of this rain belt.

(*e*) *Rainfall of* 14 *February in relation to dynamical theory*

The positive values of Sutcliffe's expression for cyclonic development have been computed for the period 09 GMT 14 February to 09 GMT 15 February 1950, and Fig. 11 has been constructed to show the rainfall equivalent of the cyclonic development integrated with respect to time. The calculation followed the lines indicated in Section 2 (*e*). Fig. 11 shows values of a little under 5 mm over eastern and southern England increasing to about 12 mm in the region of the Hebrides. The values for England agree well with those found at places where there is no significant orographic effect. It is difficult to make a satisfactory comparison over Scotland because of the widespread orographic effects, but the amounts of 12 mm measured on Colonsay and Islay, and 10 mm on Iona and Tiree suggest that the band of heaviest precipitation was again south of the area in which it would have been expected.

(*f*) *Orographic effects in the rainfall of* 14 *February*

On this occasion, also, the warm air mass had a wet-bulb potential temperature of about 50°F, and it is possible again to take a saturated adiabat through 50°F as a representative sounding in the frontal rain area. When the frontal rain area crossed Wales and southwest England the wind at 900 mb was about west by south 40 kt;

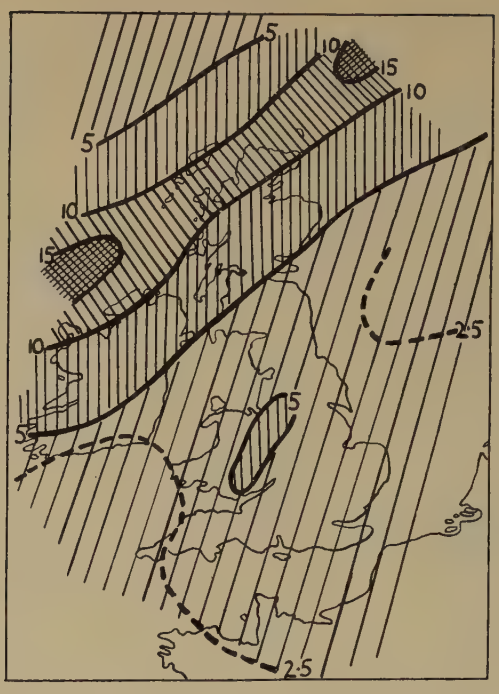


Figure 11. Rainfall equivalent of the integrated development terms 09 GMT 14 February 1950 to 09 GMT 15 February 1950.

the duration of the frontal rain was about four hours at coastal stations in SW. England and five hours on the Welsh coast. In the warm sector the wind at 900 mb was about west-southwest 50 kt, and, although there was little rain in the warm sector at low-level stations, a further 7 or 8 hours' rain occurred at high-level stations before the raingauges were read at 09 GMT. About 55 per cent of the total rain at high-level stations fell in the frontal rain area and most of the remainder in the warm sector (some showers had occurred in the Welsh hills early on 14 February and were included in the total for the day). The probable orographic addition to the frontal rain has been computed in a manner similar to that adopted in the preceding example. Devon may be treated as a barrier 1,000 ft high, and the rain assumed to spread over 35 mi; the additional orographic component of the rainfall is then 6 mm. Similarly, if Wales is treated as a barrier of 2,000 ft and the rain is assumed to spread over a depth of 35 mi, the orographic increment to the frontal rain is 12 mm. It is not certain how much of the observed rainfall fell in the frontal rain belt and how much in the warm sector, but from the autographic records it seems that practically all the rain fell in the frontal belt at low-level stations and about 55 per cent at high levels – say an overall percentage of 80 per cent. On this basis an average of about 7 mm of rain fell in the frontal rain area over the Devonshire hills of which about 3 mm can be regarded as the orographic increment. Over the Welsh hills an average of about 12 mm fell in the frontal rain area, of which about 8 mm should be regarded as due to orographic effect. It thus appears that the computation has over-estimated the effect of the orographic lifting – a closer agreement would have been achieved if the lifting had been assumed to extend up to only 700 mb instead of 500 mb.

An examination of the radio soundings at Camborne and Larkhill in the warm ector shows that the air was moist up to about 800 mb but above that level it was elatively dry. It is probable that further north the moist air was deeper. However, aking the Camborne soundings as typical of the warm air we find that if the undisturbed ayer is assumed to be 500 mb orographic lifting of the lower layers by 1,000 ft would esult in the condensation of approximately 0·06 cm of water and orographic lifting y 2,000 ft would result in condensation of 0·12 cm of water. With an undisturbed ayer at 700 mb the figures are about 0·04 and 0·08 cm of water. Taking the undisturbed ayer as 700 mb, a wind of 50 kt, and a spread of orographic rain over a depth of 35 mi t a 1,000 ft barrier we obtain a rate of rainfall of about 0·5 mm hr^{-1} or at a 2,000 ft arrier of about 1·1 mm hr^{-1}. At Princetown (1,360 ft) the average rate of rainfall n the warm sector was about 0·5 mm hr^{-1} during the period in which measurable ain fell (about 2/3 of the warm sector). At the Welsh stations, Taf Fechan, Lake Vyrnwy and Cym Dyli the rainfall averaged about 1 mm hr^{-1} in the warm sector; hese figures conform closely with the preceding estimates.

The absence of any marked orographic effect on the southern Pennines is noteworthy; the air had already been lifted to a similar height over Wales. The absence of any marked rain shadow between the two masses of hills suggests that the air did not descend significantly between them.

The general rainfall over the Lake District can be adequately explained in a manner similar to that over Devon and Cornwall but the very heavy rainfall in the centre of the area would require a study of local topography to be fully understood.

The rainfall distribution over southwest Scotland is unexpected and difficult to nterpret. The maximum of over 40 mm fell at Greenock, which is close to sea level, as well as on higher ground to the southeast. The point of occlusion passes near to his area and the analysis of the rainfall is therefore complicated. Moreover, it seems unlikely that the explanation is to be sought on general dynamical grounds. The role of the fronts seems rather to have been to maintain a deep damp air mass and general rainfall over the area for a longer period than elsewhere, thus enabling a heavy orographic fall to accumulate. Douglas and Glasspoole (1947) have emphasized the importance of deep damp layers of air and continued rain in a warm sector as factors n producing heavy orographic falls. Rain continued at both Renfrew and Eskdalemuir for over 10 hr. At Prestwick also, where the total fall was quite small, and orographic effects presumably not important, the rain was spread over a similar period. However, although the total amount of rain which fell over southwest Scotland could probably be adequately explained on the same lines as those adopted for Devon and Wales assuming a duration of 10 hr, the peculiar concentration around Greenock would probably need a careful study of local winds and of other similar cases before any definite conclusions could be reached.

The areas deficient in rainfall are again not distributed in any very obvious relationship to the high ground. Their locations over England are, however, rather similar to those which occurred on 14 March 1949; there are dry areas round Gloucester and eastward from Bristol to the Thames Valley and Salisbury Plain. The small dry areas east of Stoke-on-Trent and near Hemel Hempstead are surprising.

4. Conclusions

The study of the rainfall distribution on these two occasions suggests that despite minor irregularities in the recorded rainfall the general distribution over the British

Isles on these occasions can be analysed into two components – dynamical and orographic
The occasions were selected to exclude, as far as possible, rainfall of a convectional or
instability character; the results can therefore be regarded only as suggestive of the
structure of steady rains. The dynamical component moved across the country with
the pressure system with which it was associated and appears to have maintained a
broadly recognisable structure. The total amount of the rainfall can be adequately
explained on the basis of Sutcliffe's theory of development.

The orographic component of the rainfall arises from lifting of the moist air by
ranges of hills. In association with the lower hills (not exceeding 1,000 ft) and on
the approaches to the higher ranges, the orographic component occurs primarily within
the rain areas already established by dynamical causes and augments the rainfall
there. The amount of this orographic rain is consistent with orographic lifting extending
upward to a level of 700 or 500 mb. These conclusions are in general agreement
with those of Douglas and Glasspoole.

The distribution of areas deficient in rainfall (rain shadows) is not easily explained
in relation to the high-ground masses. The diminution of rainfall east of the main
barriers appears to be less than would be expected if the air descended again to the
extent to which it had been lifted.

Acknowledgments

The author wishes to acknowledge the assistance of the Climatology Division
of the Meteorological Office in the preparation of Figs. 6 and 10, and the help of the
staff of the Forecasting Research Division in the extraction of the data from the
autographic records. Acknowledgment is also made to the Director of the
Meteorological Office with whose permission the paper is published.

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551.555.9 : 551.509.317

A technique for wind analysis and forecasting as applied to Victoria, British Columbia*

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(Manuscript received 16 October 1951, in revised form 28 December 1951)

Summary

Through the use of statistics the most frequent wind directions at Victoria are isolated and their causes considered. Charts of the seasonal variation in wind direction are constructed and the causes for these changes discussed. Finally the data are developed to permit quantitative forecasting of the wind direction at Victoria and the probabilities of the accuracy of the forecasts are calculated.

1. Relationship between the geostrophic and surface-wind directions

The main factors which affect the surface wind are : pressure gradient, terrain, land and sea effects, and anabatic and katabatic effects. The situation of Victoria on Vancouver Island on the rugged west coast of British Columbia brings all these effects into play. It may therefore be helpful if we give a short description of the geography of the district and of the major wind effects.

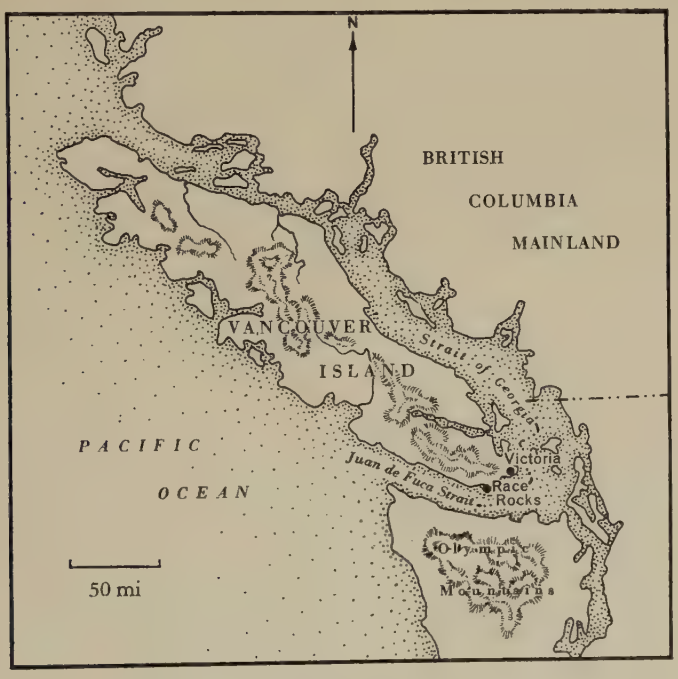

Figure 1. Geography of the Victoria area.

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In Fig. 1 are shown the position of Victoria and the important geographical features of that region. Of paramount importance is the Olympic mountain range which extends approximately east to west along the southern bank of the strait of Juan De Fuca and some fifty miles south of Victoria. Also of great importance is the range which forms Vancouver Island. The only other significant land body is the North American mainland lying to the east. The main bodies of water are : the Pacific Ocean, lying to the west ; the Strait of Juan De Fuca, to the south of the island and opening in the west on to the Pacific Ocean ; and the Strait of Georgia, separating southern Vancouver Island from the continent to the east. Victoria is situated on the southeast corner of the island, about eleven miles to the northeast of the Race Rocks, which lie at the eastern end of the Strait of Juan De Fuca. The mountains bordering this strait form a ' funnel ' through which, under favourable conditions, air from the Pacific flows. These winds reach Victoria as southwesterlies because of the bay separating it from Race Rocks. The geostrophic wind responsible for the pouring of Pacific air through the funnel is approximately northwesterly, a condition which occurs when pressure is high in the Pacific. For this reason southwesterly winds are experienced with northwesterly geostrophic winds. In the afternoon this movement of air is supplemented by the sea breeze. The rapid heating of the North American continent during the day creates a circulation which causes Pacific air to move towards the land in the surface layers. At Victoria the motion of the sea breeze is from the southwest since the bulk of the Pacific air must come through the funnel of the Strait of Juan De Fuca. In the same manner southeasterly geostrophic winds and the night land breeze tend to force air outwards through the straits to the ocean. The flow of air over Victoria as it heads for the funnel results in a northeasterly wind. Thus the controlling factor of the air flow in the lower levels in the region of Victoria is the funnel effect of the Strait of Juan De Fuca.

These facts may also be demonstrated statistically. The approach is similar in some respects to that described by Leipper and Miller (1946). The variable factors which offer the most promise in forecasting the wind direction are the geostrophic-wind direction and the diurnal effects resulting from unequal heating of the land and sea masses. With this in mind a comparison was made of the geostrophic wind, as measured from the sea-level map, and the corresponding observed wind at Victoria. In order to eliminate, as far as possible, the diurnal factors the data were grouped according to the time of day.

All the surface charts for the two-year period from April 1941 to March 1943 inclusive were examined. The gradient wind directions at Victoria were obtained from the charts with the aid of a protractor and were classified according to the sixteen-point compass. Only reliable directions were recorded, all doubtful or indeterminate cases being rejected. From the Victoria records of hourly observations the surface-wind direction, taken at the synoptic hour, was read off and recorded with the appropriate geostrophic wind. In order to obtain significant wind directions only winds of Beaufort force two or greater were used. Accordingly, when the wind speed was indicated as 3 mi/hr or less the wind was listed as calm. From the 2,920 maps examined suitable geostrophic wind directions were extracted from 1,570, or over 50 per cent. This provided approximately 400 cases for each of the four synoptic hours, a sufficient number to provide reliable information regarding the relationship between the geostrophic- and surface-wind directions. All times referred to are Pacific Standard.

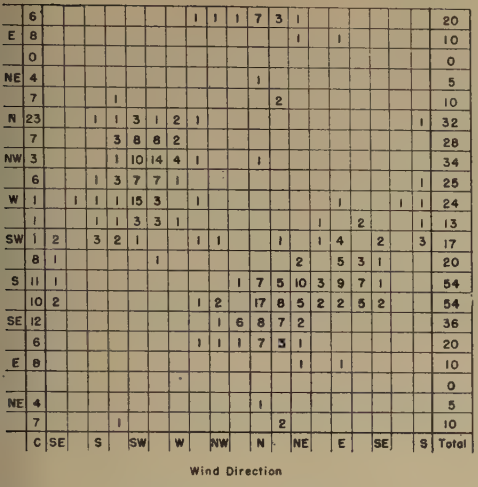


Figure 2. At 0430 PST.

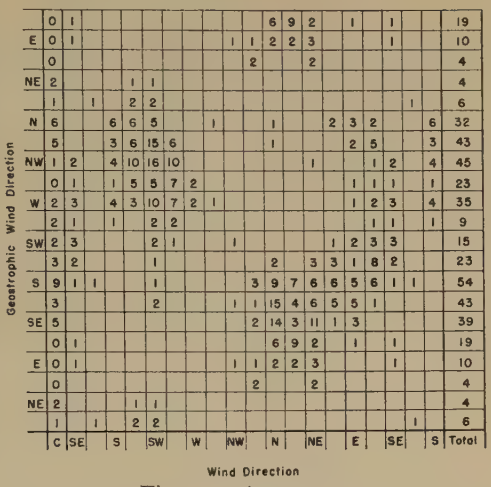


Figure 3. At 1030 PST.

Figures 2 and 3. Distribution of geostrophic and observed wind directions at Victoria.

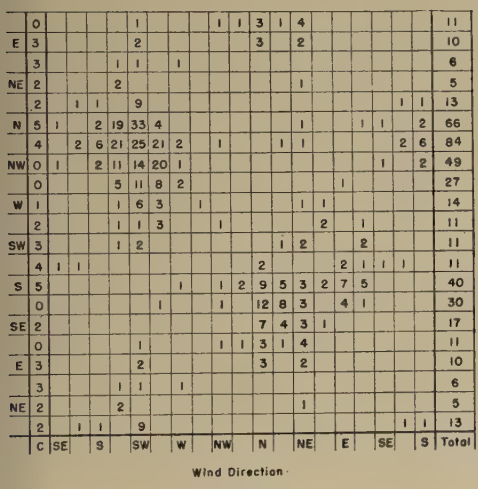


Figure 4. At 1630 PST.

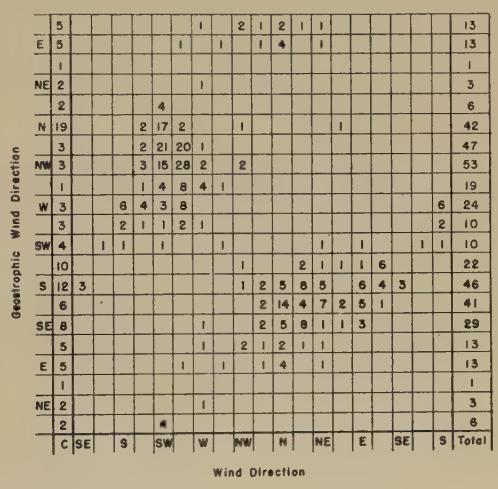


Figure 5. At 2230 PST.

Figures 4 and 5. Distribution of geostrophic and observed wind directions at Victoria.

In Figs. 2 to 5 the data are plotted respectively for the times 0430, 1030, 1630, and 2230 PST. The abscissa is the surface-wind direction, with calm winds (0 to 3 mi/hr) listed in a separate column to the left. The ordinate in each figure is the geostrophic-wind direction. In a column at the right is listed the total number of geostrophic winds measured for each direction. Since the data repeat themselves in either direction, extension of the charts, by repeating some rows and columns, has been necessary to indicate the groupings.

The most important figures to be noted are the consistent groupings of the southwesterly and northeasterly winds corresponding to geostrophic winds from northwesterly and southeasterly directions respectively. These groupings are found on all four charts and it is obvious that any factors of persistence which may have been present

in consecutive data must be insignificant. The statistics, therefore, agree with what would be expected from experience and a knowledge of the local geography, with special reference to the funnel effect in the Strait of Juan De Fuca ; a northwesterly geostrophic wind corresponds to a southwesterly surface wind and a southeasterly geostrophic wind corresponds to a northeasterly surface wind. Especially apparent at 1630 hr is the effect of the sea breeze which assists the southwesterly wind. At this time the wind usually blows from the southwest even with northeasterly gradient winds. The charts also show the scattering of surface winds corresponding to geostrophic winds from directions intermediate between northwesterly and southeasterly. This information is already known to the forecaster in a qualitative way but the charts bring out these facts clearly and indicate the critical points between the major directions.

2. The forenoon easterlies

An interesting secondary grouping of easterly winds, corresponding to northwesterly geostrophic winds, is noticeable at 1030 hr (Fig. 3) and to some extent at 1630 hr (Fig. 4) but is almost nonexistent at 0430 hr and 2230 hr (Figs. 2 and 5). The descriptive term ' forenoon easterlies ' has been applied to this group. Although this group is numerically smaller than the southwesterly wind group corresponding to the same gradient wind it is very definite and requires some explanation.

The class interval of one compass point employed heretofore is somewhat too narrow for a study of the broad features of the group. It is more satisfactory to focus attention on the main directions involved. Thus we restrict our data to northwesterly geostrophic winds, selecting those directions between west-northwest and north, inclusive. For the four synoptic periods the data corresponding to these geostrophic winds were extracted from Figs. 2 to 5. The observed winds were then placed into broad groups : calm winds ; easterlies, defined to include all directions from east-northeast to southeast ; and southwesterly winds, defined by the following directions, at 0430 hr and 2230 hr, south-southwest to west ; at 1030 hr and 1630 hr, south to west-southwest. The slight difference in definition for various synoptic hours is necessitated by the groupings of the data which display a slight diurnal variation in the direction of the southwesterlies, due to differential heating effects. Not all of the observed winds fit into these three groups but the percentage excluded is insignificant. Table I shows the result of this classification.

TABLE I. Percentage of winds from various directions when the geostrophic wind is northwesterly

Time PST	Calm	Easterly	Southwesterly	Total
0430	33	0	63	96
1030	8	14	74	96
1630	4	2	89	95
2230	16	1	81	98

Table I shows that the percentage of calm winds is high at 0430 hr and diminishes about 25 per cent by 1030 hr. During the same period the number of easterly and southwesterly winds increases by 14 and 11 per cent respectively. From this it appears that a substantial number of the calm or very light winds of the night develop into

sterly winds by 1030 hr, and a slightly lesser number into southwesterlies. By 1630 hr e southwesterly group increases by 15 per cent mainly at the expense of the easterlies. his would indicate that the forenoon easterlies are rather light winds. At 2230 hr, the absence of the influence of surface heating, the calm group has increased 12 per nt though the percentage of southwesterly winds remains rather high.

The forenoon easterlies are evidence of a sea breeze from the Strait of Georgia to ancouver Island. The fact that Victoria is separated from the mainland by about fty miles of water suggests that the continental sea breeze would not be felt at 1030 hr. y mid-afternoon, however, the sea breeze from the Pacific Ocean to the continent ecomes dominant and the minor sea breeze to the island is overwhelmed by the ain flow of air towards the mainland. As a result of the funnel effect of the Strait f Juan De Fuca this flow appears as a southwesterly wind at Victoria.

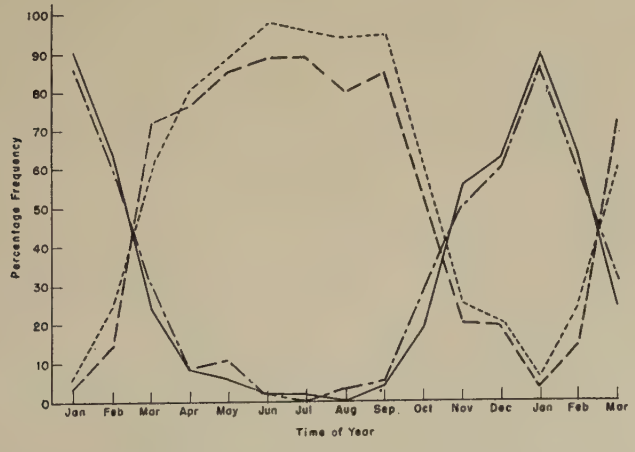
3. Seasonal variation of wind direction

It is of some interest now to see how the winds and their corresponding pressure atterns are distributed through the year. Considering the two important observed ind groups and their associated geostrophic-wind groups found in Figs. 2 to 5 is convenient to select the following limits to the bands :

northeasterly surface wind – north to east ; southwesterly surface wind – south to west ; southeasterly geostrophic wind – east to south ; northwesterly geostrophic wind – west to north.

Neglecting cases of calm the percentage of data falling into each group was computed r each month of the year and the results are shown in Fig. 6. The data for January, ebruary and March have been repeated at the right in order that the sharpness of he January peaks and troughs may be more readily seen.

The two surface-wind groups contain practically all the winds that occur at ictoria since their sum is close to 100 per cent. Similarly the two geostrophic-wind roups contain a very high percentage of the gradients. The most striking feature



igure 6. Seasonal variation in frequency of geostrophic and observed wind directions at Victoria.

——— Southeasterly geostrophic wind. — — — Northwesterly geostrophic wind.
— - — - Northeasterly actual wind. - - - - - - Southwesterly actual wind.

of the chart is the close correspondence, which has been noted previously, between the northwesterly geostrophic wind and the southwesterly surface wind, and between the southeasterly geostrophic and the northeasterly surface wind.

The seasonal variation in the geostrophic wind is apparent at once from Fig. 6. The percentage of the northwesterly geostrophic winds and their corresponding southwesterly winds is at a minimum in the month of January and is in fact, less than about 20 per cent of the total between the months of November and February inclusive. From March to September inclusive, the percentage of geostrophic and observed winds from these directions is very high with a sharp rate of rise from February to March and a sharp fall from September to November. Similarly, the percentage of southeasterly geostrophic and their corresponding northeasterly surface winds is low from March to October inclusive, rising fairly steeply to a peak in January, then falling steadily again. The points where the two sets of curves cross may be taken as dividing summer from winter winds. This means that, considering only two seasons throughout the year, Victoria has summer winds for eight months from March to October and winter winds for the four months from November to February inclusive. Of the two seasons summer is the more consistent since the wind curves are fairly flat during these months whereas the winter curves reach a decided peak or trough in January.

The reason for this summer and winter variation in the direction of the gradients and winds at Victoria is to be found in the movements of the Pacific high-pressure cell and the winter development of waves on the eastern Pacific Polar front. In summer the Pacific high-pressure cell is well developed with a preponderance of northwesterly geostrophic winds along the British Columbia coast and consequently a preponderance of southwesterly winds at Victoria. In wintertime the Pacific high no longer dominates the pressure pattern in these latitudes and pressure is normally higher over the land than over the sea. As a result the tendency is for air to flow out through the Juan De Fuca funnel to the sea giving rise to northeasterly winds at Victoria. At this time of year the eastern Pacific Polar front is well developed and waves forming on this front move over the British Columbia coast in regular progression. Convergence associated with these systems together with the increased gradient along the coast tend to intensify the flow of air seawards. Since most of this air in the surface layers must flow through the funnel, northeasterly winds become general at Victoria.

Thus, the seasonal variation in wind at Victoria is the result of the seasonal variation in the Pacific high. The intimate connection between the movements of pressure systems and the wind at Victoria suggests that the wind is also closely related to the weather at Victoria and, indeed, in the Pacific Provinces and States. If the weather and wind direction are actually as closely related as our reasoning suggests then Fig. 6 might be considered as an index of the seasonal variation of summer and winter weather in western North America.

5. Use of statistics in forecasting the wind direction

In this section the statistics will be developed into a form which may be used directly in forecasting, on the assumption that it is easier to forecast the pressure distribution than it is to make a direct forecast of the wind. As an example of the method consider the data in Fig. 4. Suppose the information is given that the geostrophic-wind direction falls in the set consisting of the two directions north and north-northwest so that the total number of cases is 150, and the number of calm cases is 9.

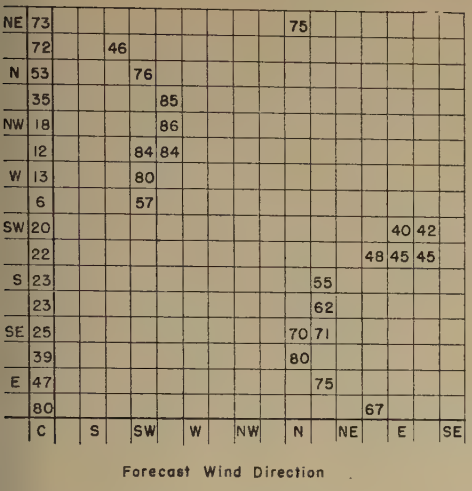


Figure 7. At 0430 PST.

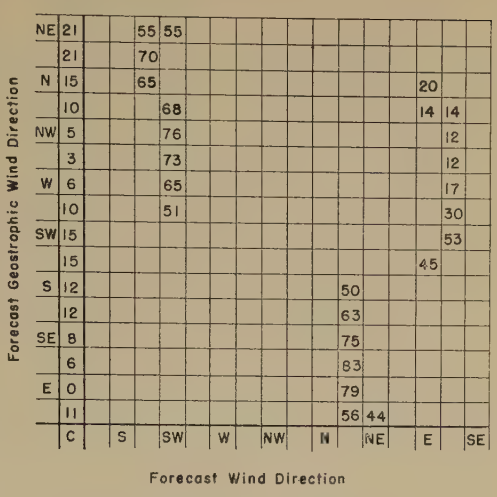


Figure 8. At 1030 PST.

Figures 7 and 8. Forecast probabilities of observed wind direction for a given geostrophic wind direction.

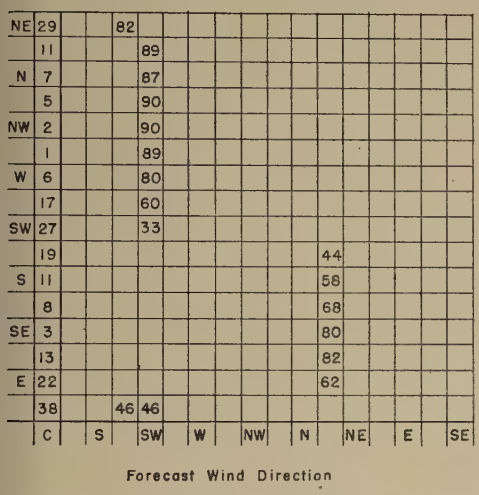


Figure 9. At 1630 PST.

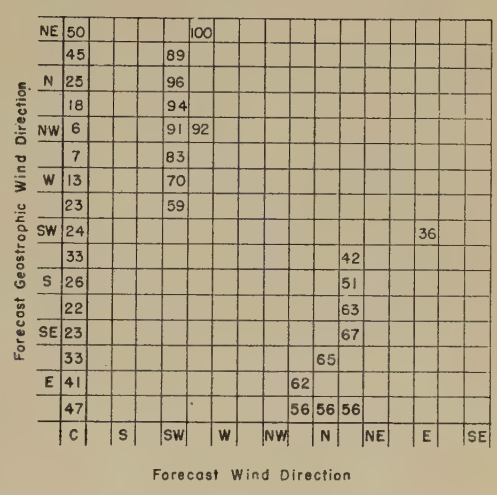


Figure 10. At 2230 PST.

Figures 9 and 10. Forecast probabilities of observed wind direction for a given geostrophic wind direction.

Suppose further that the set of observed wind directions in which we are interested consists of the five directions from southwest to southeast through south. The number of events indicated by the rectangle of intersection is found to be 109. It is obvious that the statistical probability of a calm given that the geostrophic wind is either north or north-northwest is $p = 9/150$ or 6 per cent. Also the probability, under the same geostrophic conditions, of a wind direction in the band from southwest to southeast is given by $p = 109/150$ or 73 per cent.

As a refinement it is perhaps better to consider the probabilities of calm winds apart from wind direction. It may happen that 90 per cent of the winds corresponding to some given geostrophic-wind direction are calm and the remaining 10 per cent are

from a single direction. In such a case it would be misleading to state that the probability of a wind from that direction is 10 per cent whereas in reality if a wind exists at all the statistical probability of its having this direction is 100 per cent. For this reason we will indicate the probabilities of calm winds as percentages of the number of favourable geostrophic winds. The probabilities of observed wind directions will be given as percentages of the totality of non-calm winds.

There remains only the consideration of suitable intervals for the sets of geostrophic and observed winds. The choice of the geostrophic interval must depend on what may reasonably be expected of a forecaster in predicting the pressure distribution. This in turn depends on the experience and ability of the forecaster, the location of the station, the degree of refinement of the techniques used, the time interval, and so on. For Victoria it seems that a forecast of the geostrophic wind which would verify within one compass point on the sixteen-point compass may reasonably be anticipated and this interval was used in our computations. A forecast within one compass point is as close as may be expected for the surface wind and is certainly sufficient for all normal purposes. For the computations presented here the intervals of both geostrophic wind and surface wind have therefore been taken as three points. The probabilities computed from the data in Figs. 2 to 5 are shown in Figs. 7 to 10.

Some of the facts presented in these probability charts are worthy of special note. That southwesterly winds result from northwesterly geostrophic winds is once more clearly illustrated in the high probabilities for forecasts of these directions, usually over 80 per cent. The probabilities of southwesterlies at 1030 hr are somewhat lower owing to the significant easterly group which occurs at that time of day. The charts also determine the best forecast direction much more closely than the general statistics considered earlier. The charts indicate with clarity the critical geostrophic directions which fall between the northwesterly and southeasterly directions. At these points the probabilities fall considerably.

Evidence of the sea breeze from the southwest is indicated by the somewhat higher percentages of the forecasts for this direction at 1630 hr over 0430 hr. Further evidence is provided by the fact that major forecasts of southwesterly winds are indicated by 10 of the 16 geostrophic wind directions at 1630 hr as compared with 7 of the 16 directions at 0430 hr.

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Thomas Barker's Meteorological Journals, 1748-1763 and 1777-1789

By GORDON MANLEY
Bedford College, University of London

(Manuscript received 9 January 1952)

In the Library of the Royal Meteorological Society there is a considerable number of lder meteorological records kept at various times in different parts of the British Isles ; ney form indispensable source-material. Some account of the problems met with in the eduction of such records to present-day standards will be found in the writer's discussion f the trend of Lancashire temperatures since 1753 (*Quarterly Journal* 1946). Rainfall data ave been discussed by Glasspoole (*Meteorological Magazine* 1928, *Quarterly Journal* 1931). fforts have often been made to provide long-continued tabulations of other elements, e.g., ne frequency of snow in London was the subject of a short paper by Symons (*Quarterly ournal* 1896) which one can enjoy re-reading on account of the highly critical discussion hich followed. Tables of the actual daily observations are very necessary when it is a question f assessing the qualities of the instrumental exposure, the nature of the errors arising with ld-fashioned instruments, and the alertness of the observer with regard to eye observations ; nis is especially true of all records before 1800.

Several long British records of temperature have been built up giving monthly means since ome date after 1752. For example, the London temperatures, reduced to the standard of the Greenwich 24-hourly means, are available back to 1763 (Brunt 1925) and an Edinburgh eries goes back to 1764 (Mossman 1897 and 1900). By mischance it has long appeared that n southern England no observations of that element for the year 1762 have survived. While nere are some quite good London observations from 1755 to 1761, these could not be bridged nto subsequent series. In Scotland there is an indoor-exposure record from near Blairgowrie, hich if satisfactorily reduced might provide a few years' rough extension of the Edinburgh ables which could then be taken back to 1758.

I have however recently investigated two of the MS. diaries noted by Mr. C. E. Britton n a series of summaries which he prepared for the Society's Library between 1925 and 1930. One of these, now in the Society's possession, was kept by S. Milford at Exeter (1755-1774) nd while it is not by any means useless scrutiny soon reveals its many imperfections. The econd, from the library of Lancing College, is far more interesting, being one of the manuscript iaries kept by Thomas Barker, the brother-in-law of Gilbert White. Barker lived at Lyndon Hall in Rutland ; the house, 320 ft above sea-level, still stands and the location can be observed n detail, with a southerly exposure on a well-defined ridge extensively planted with trees iving shelter to the house and grounds. This diary covers the period March 1748 to August 763, with relatively little interruption ; it includes twice-daily readings of temperature, also ressure, rainfall, wind direction and strength, and remarks on the weather. It thus provides n opportunity to bring the monthly means for the period before 1752 to values appropriate o the ' New Style ' Calendar. This change of the calendar by eleven days was only adopted n England in September 1752, whereas it had been operative in neighbouring countries nce 1700 or earlier. Hence if any comparison is to be effected between English and Dutch, erman or French monthly means from these early days of thermometry the calendar adjust-ent must first be made.

Barker read his old ' Royal Society's pattern ' thermometer, which was made by Patrick, sed spirit of wine, and was nearly 3 ft long, in an indoor room facing north, where no fire was ade. The extremes of temperature show that the room was well ventilated. At intervals e read other instruments out of doors, and in 1763 he kept one month (January), ' in ' and out.' In general, the thermometer was read twice daily, early in the morning and in mid-fternoon. In 1758 he re-graduated his instrument to the Fahrenheit scale and, on the sumption that there was no change, for example, in the qualities of the glass, we can extend ackward the Fahrenheit equivalents of the indoor monthly means. There is a further volume Barker MS. daily observations 1777-1789 in the Society's Druce Collection. These

evidently provide part of the basis for the later series of " indoor ' and ' outdoor ' mean from Lyndon which he published in the *Philosophical Transactions* from 1771-1798. It might be thought that suitable corrections for his earlier observations could be derived therefrom. This is not the case ; he broke his outdoor thermometer in 1786 and a close comparison of the differences indicates a discontinuity arising, very probably, from a difference of nearly 2°F between the older and newer ' outdoor ' instruments. We do not know what happened to the instruments between 1763 and 1771, or whether he used the same outdoor exposure throughout. Moreover there is insufficient overlap with any homogeneous London record to enable one to be sure that the earlier and later Barker series can justifiably be integrated, however probable that might appear.

Hence I have treated this earlier Barker record as an independent series. Inspection shows that the general character of the difference between ' indoor ' and ' outdoor ' monthly means is consistent, and that they vary from about 5° in a severe midwinter month to zero in an average summer. I then took out the mean *daily* temperatures recorded by Barker on days of very exceptional summer heat, and on days when sleet was observed, together with a few other cold, windy and cloudy days and entered them on a graph (Fig. 1) against the ' probable outdoor mean ' which experience would lead one to expect with such wind direction on such days. For example, we are aware that at a ridge site in the Midlands a day with a mean temperature of 75° is very exceptional. It would imply a maximum of 90° with a minimum 60° ; and a little scrutiny of past records will show that it is rare for such figures to be reached at places like Rugby (390 ft) or Belvoir Castle (259 ft) which lies a few miles north of Lyndon. Taking the properties of air masses into account I think it probable that the extreme limits of warmth experienced in the eighteenth century do not differ materially from those in the present century. This conclusion is supported by the fact that whereas at Durham 87° has been reached on several occasions since 1920, in a hundred years that figure has only twice been exceeded and 89° is the highest maximum since 1847. Other long records, e.g., Greenwich, lead to similar conclusions.

The uppermost points (A) in Fig. 1 each refer to the last of a sequence of exceptionally hot days with continental air, in July 1749, 1750 and 1757, just preceding the characteristic thundery break-down or the arrival of air from the west. The points BB relate to a variety of other hot days between June and September for which the diary record enables one to give an estimate ; these are likely to be less reliable. The points CC refer to the cold, snowy or sleet days. Imperfections of estimation, together with the usual variation in lag likely to occur in autumn and spring between the daily means indoors and out, should result in points derived from individual days being somewhat scattered. But a line drawn through the two clusters should represent, quite reasonably, the relationship between the monthly means indoors and out. Accordingly I have drawn the curve to pass through two additional points. Point D refers to the coldest month in the series (January 1763) for which Barker's

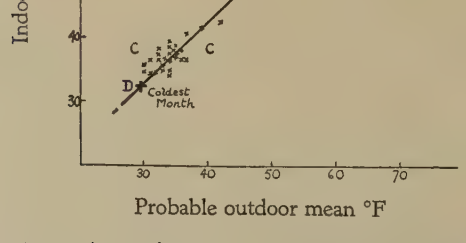

Figure 1. Lyndon 1748-1763. Approximate conversion diagram of indoor to outdoor temperatures

bserved outdoor mean, allowing for the conditions of observations, appears to lie close to
·5° ; this is supported by the London mean for the same month, based on the Greenwich
duction, of 31·0°. The point E is based on the overall outdoor mean temperature at Lyndon
r the sixteen years in question which I have estimated as approximately 48·3°, for reasons
ven below ; the overall indoor mean from all Barker's readings being 50·4°.

With this reinforcement we should be able to read off approximate outdoor equivalents
r each indoor monthly mean. For the warmest month (July 1757, indoor mean 67·0°) the
utdoor mean becomes 65·5° ; for the very cold month of January 1757 the outdoor mean
ecomes 31·7°.

We have some additional check on these values. There are two London series, kept
f Fleet Street and Ludgate Hill respectively by rival instrument makers, obviously in very
eltered city yards or perhaps small shaded gardens ; this is very evident from the appearance
f the summer afternoon temperatures. Two daily readings (early morning and mid-afternoon)
e available using improved Fahrenheit thermometers. Both point to a monthly mean between
5° and 67° for July 1757. For the same month in the north a useful Carlisle record indicates
mean close to 63°. From other contemporary observations the month was generally dry,
ot and burning,' and for a time not unlike August 1947 from the point of view of the
essure and temperature distribution. We also know that in recent years the highest monthly
eans for this type of exposure and altitude at rural stations in the Midlands have been of
e order 65·5°. For all these reasons the value 65·5° for July 1757 must lie near the truth ;
oreover there is independent support from Holland as well as Exeter.

For January 1757 the approximate means are 31° at Carlisle and 35° in *inner* London
ith *about* 34° at Exeter ; allied with Barker's notes on the weather, these would suggest
at the value derived, namely 31·7°, for his Midland location is reasonable.

It remains to comment on the reasons for the ' overall mean ' of 48·3°. Barker was a
cidedly careful observer of snow and sleet as his text indicates and the overall frequency
f days with snow or sleet observed, making allowances for breaks in his own record from other
ntemporary records, is 21·5 days per annum. From his text I regard his observations as
f ' good standard,' quite up to that of certain of the better climatological stations now available
ough probably not quite so alert as, for example, Cranwell. Over the period 1912-1938
t Lyndon it would appear that the average annual frequency was probably just over
venty days (Oxford, 18·2 ; Mayfield, 24·3 ; Oundle, 15·2 ; Belvoir Castle 1912-1930,
7·5). I think it reasonable therefore to assume that the overall winter mean and also the overall
nnual mean for 1748-1763 was a little below that for 1906-1935. For this period a considera-
on of the available data from ten neighbouring Midland stations, reduced to a mean based
n daily observations at an evening hour, and the character of the Lyndon site, suggests an
annual mean ' (1906-1935) close to 48·5°. For 1748-1763 there are other independent
ntemporary snow records which support these conclusions.

From Table I the winter mean (November to April) comes out as 40·4° compared with
1·1° for 1906 to 1935 ; the annual mean is slightly below the modern average ; the summers
ken overall are slightly warmer. This is all in excellent accord with the contemporary trend
own by Labrijn's tables for Utrecht in Holland which point to an appreciably greater
nuary-July range or increased ' continentality '. It is noteworthy that these earlier ther-
ometers were probably decidedly sluggish. In a letter in the Royal Society's MSS.
Tettleton of Halifax describes his instrument as being 2 ft long (somewhat like Barker's)
ith a bulb 1·6 in. in diameter. Hence, although Barker does not mention the size of the bulb,
was presumably similar. Thus Barker's extreme readings are of interest ; he has a number
f entries for exceptional days according to which his ' outdoor shade ' reading was approxi-
ately 88° on 15 July 1750. Allowing for the well-shaded grounds and the presumed qualities
f the thermometer, this agrees well enough with recorded extremes at neighbouring ' upland '
ations during the present century. He also made particular mention of what he regarded as
remarkable outdoor reading of 55° or thereabouts on 22 December 1753, following a spell
f cool weather earlier in the month ; present-day records suggest 57°-58° as a probable
xtreme for December. With regard to his later journal (1777-1789) I took out from the curve,
sing the ' indoor ' records, the approximate outdoor mean for the very hot July of 1783 ;
e result, 65·8°, slightly exceeded that of July 1757 and was again in full agreement with the
Utrecht series, according to which July 1783 just surpassed any other in the last 246 years.

In view of the sluggish thermometers, the doubtful outdoor exposure and the variable hours of observation, there is much to be said for direct derivation of monthly means from the indoor 'cool-room' observations. Nevertheless it must be agreed that the above reduction process probably gives rise to a slight diminution of the springtime monthly means and a corresponding exaggeration of those of autumn, owing to the lag arising from the nearby walls of the house. There is no very good way of allowing for this, other than approximately. Inspection of the 'indoor' and 'outdoor' means for 1771-1786 supports this view, but equally it must be remembered that radiation effects on imperfectly exposed thermometers, bearing in mind his hours of observations, would tend to exaggerate the spring 'outdoor' means compared with those of autumn. From a consideration of these differences, the hours of outdoor observation, and the available snow data, I think that if an addition of 0·5° is made to the means for April and May derived from the curve, with a corresponding subtraction in October-November, this error arising from lag will largely be eliminated. It is reassuring to find that if Barker's indoor readings for 1777-1789 are similarly reduced, the resultant outdoor means agree well with other contemporary series and also with the observed frequency of snowfall.

TABLE I. Lyndon, Rutland. (320 ft) approximate monthly means, 1748-1763
(New Style *throughout*)
(Individual monthly means have been rounded to the nearest 0·5°)

Year	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.	Mean
1748	35·5	34·5	35·0	43·0	50·5	59·0	60·0	60·5	57·5	48·0	44·0	42·5	47·5
1749	40·5	37·0	41·0	43·5	54·0	53·0	63·0	60·0	57·0	50·0	43·5	39·5	48·5
1750	38·5	43·5	46·0	45·5	51·0	57·5	63·0	59·5	59·5	48·5	39·0	38·5	49·2
1751	38·5	34·0	43·0	44·5	48·5	59·0	60·0	58·5	55·0	46·5	39·0	36·5	46·9
1752	37·0	36·5	41·5	44·0	50·5	59·0	60·5	60·5	57·0	50·5	43·0	38·5	48·2
1753	35·0	38·0	42·5	45·5	54·0	58·5	59·5	60·5	55·5	49·5	39·5	39·0	48·1
1754	37·0	36·0	37·5	44·0	54·5	56·5	59·0	60·5	57·0	50·5	41·5	37·5	47·6
1755	35·0	33·5	38·5	50·0	49·0	60·5	59·5	58·5	56·5	46·5	40·0	38·0	47·1
1756	39·0	39·5	42·0	44·0	48·5	57·0	61·5	58·5	57·0	48·5	38·0	36·5	47·5
1757	31·7	38·5	40·5	47·0	51·5	57·0	65·5	60·0	56·0	46·0	44·0	37·0	47·9
1758	36·0	38·0	41·0	45·0	57·0	58·5	57·5	62·0	53·5	46·0	42·5	38·0	47·9
1759	41·5	41·5	42·5	47·5	54·0	59·0	65·3	62·0	56·5	51·5	40·5	36·0	49·8
1760	34·5	38·5	43·5	49·5	53·0	59·5	63·0	60·5	60·5	48·0	41·5	42·0	49·5
1761	41·0	42·0	44·0	49·0	53·5	57·5	60·5	61·5	57·5	48·5	42·5	39·0	49·7
1762	39·5	38·5	38·5	50·0	55·5	62·5	64·5	59·5	56·5	45 5	39·5	37·5	49·0
1763	29·5	40·0	41·5	48·0	50·5	58·5	60·0	60·0	56·5	46·5	42·0	42·5	48·0
Mean	36·9	38·1	41·1	46·2	52·3	58·3	61·4	60·2	56·8	48·3	41·2	38·7	48·3
Probable monthly means 1906-1935	38·8	38·9	41·5	45·5	52·5	57·1	60·8	60·2	55·8	49·2	42·0	39·3	48·5

TABLE II. Days with snow or sleet

	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.	Year's total: Av.
1748-1763	4·6	5·1	5·6	1·4	0·2	—	—	—	—	0·2	1·6	2·8	21·5
1777-1789	6·0	6·0	5·9	2·4	0·1	—	—	—	—	0·4	1·7	5·3	27·7
Present-day snow or sleet (based on mean of Oxford + Mayfield) for comparison													
1912-1938	4·7	4·5	4·5	2·1	0·2	—	—	—	—	0·2	1·2	3·8	21·2

Mean monthly temperatures based on the indoor means reduced as above : for comparison with the above average monthly frequency of snowfall, 1777-1789

1777-1789	35·9	37·9	39·9	45·7	52·9	58·1	61·3	61·2	57·1	49·2	41·8	37·8	48·2

It is believed that the values given in Tables I and II, in conjunction with other contemporary records now under examination, not only provide a good bridge through the gap for 1762 ; although in themselves they are undoubtedly subject to some error, they are sufficiently representative of the Midlands to be used to support further bridging into the period before 1750, for which some hitherto unused MS. observations have now been found. Moreover this Lyndon series, 1748-1763, is independent of the earlier ' Lancashire ' values which I have treated (*Quarterly Journal* 1946) and which are themselves subject to doubt unless reinforced. A complete table incorporating these and other records will be the subject of a later paper. The gaps in this Barker record fall mostly in 1751 and 1753 ; in the former year he paid a long visit to Selborne. I have filled them up with the aid of other contemporary records, by differencing the successive monthly means in each year, and from one year to the next.

Barker's journals of daily observations appear to have been written up in a series of uniform parchment-covered volumes. That which covers 1748-1763, and a second containing more general descriptive accounts of the weather of each year from 1733-1795, are now in the Library of Lancing College, and I have most cordially to acknowledge their loan over a prolonged period. I have also to thank the Officers of the Royal Society for permission to consult contemporary manuscripts in the Society's Library. There is a further volume (1777-1789), as already mentioned, in the Society's Druce Collection. There were probably three other volumes for the years before 1748, for 1763-1776 and 1789-1798 ; but I have found no hint of their whereabouts and, of course, they may have been lost. Barker's journals have indeed much value in other respects. There are many delightful comments on natural phenomena, the state of his crops and garden plants, and one derives an impression of an extraordinarily interesting and thoughtful country squire. Not least it is noteworthy that he was a vegetarian. His monthly totals of rainfall (1736-1798) are basic to our knowledge of eighteenth-century fluctuations in England. It is hoped that this reduction of his temperature record will be found acceptable by those interested in extending our knowledge of climatic vicissitudes ; further, the method may be of use with regard to other isolated series. The majority of the values in Table I have been brought to the nearest half degree; this is preferable having regard to the necessary imperfection of any reduction of the earlier eighteenth-century temperature observations.

It is of interest to note that Barker's ' General account of the weather ' covering the years 1733 to 1748 and written up in his journal, was printed by Dr. Short of Sheffield (in his *New Observations . . . on the Bills of Mortality*, 1750). Short also published a summary by months of Barker's earlier observations from 1736 onward, which is not lacking in value ; but it is tantalising not to be able to refer to the daily observations for this early period. Perhaps they will yet be found. From Short it may provisionally be estimated that the mean temperature of the phenomenal January 1740 (New Style) lay between 27° and 27·5° in the Midlands, and that the month was accordingly not quite so cold as the January of 1814 ; but, if the Old Style calendar month is used the mean was about 2° lower, and accordingly the severity of this period was about as great as one might expect so long as the North Sea remains unfrozen.

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A comparison of substandard pyrheliometers

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(Manuscript received 7 November 1951, in revised form 8 January 1952)

1. Introduction

Records of solar radiation at meteorological observatories are not made with absolute instruments. The records are standardized by reference to substandard, not absolute, instruments. In Europe, Ångström pyrheliometers are chiefly used, referred to an absolute standard maintained at Uppsala. In America the Abbot silver disc pyrheliometer is mainly used, referred similarly to an absolute standard maintained at the Smithsonian Institution at Washington. These two absolute scales are known to differ, and the extent of the difference has been the subject of considerable work. (See, for a review, Nicolet 1948).

The standardizing comparisons at Uppsala and Washington are, as far as is known, made on the sun. When the instruments are pointed at the sun, the sensitive surface is exposed not only to radiation received direct from the sun, but also to small-angle forward scattering from the atmosphere i.e., to the sun and to a certain area of surrounding sky. This area of sky differs for the two types of instrument, and its brightness varies with solar altitude and with the meteorological conditions. Whilst a simultaneous comparison of two precisely similar instruments is possible using the sun as a source, it is not possible properly to compare two instruments of different construction in this way ; they do not receive the same amount of radiation. It therefore seems reasonable to suggest that standardizing comparisons of radiometers for meteorological use, whether between the absolute instrument and the substandard, or between substandards, should be made not on the sun but in the laboratory on a precisely defined source. This argument was forcibly expressed by Guild (1937a) but has not been accepted in meteorological circles.

This note reports a laboratory comparison of Ångström and silver disc substandard instruments at Kew Observatory. It is thought to have more than local interest on two counts : firstly the light it throws on the stability of these substandards, secondly the support it lends to Guild's views.

2. Apparatus and experimental procedure

(a) *Source and screens*

The source used was a 230 v, 2,000 w gas-filled tungsten-filament lamp run at about 180 v, the voltage being hand-regulated. At best the output of the lamp varied only a part or two in a thousand during one hour, but at times there were sharp voltage changes, and the results obtained showed that a variation in total radiation output of 1 to 2 per cent in half an hour was fairly frequent. The same lamp was used throughout the work and it suffered a general deterioration in output of about 7 per cent over 6 months, during which period it was used a good deal for other work.

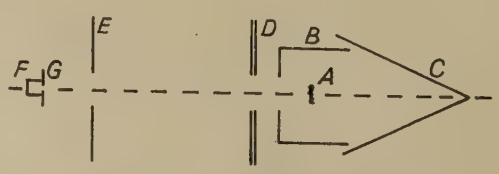

Figure 1. Arrangement of screens.

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The lamp A was enclosed in a ventilated metal case B with a black cloth C arranged so at as little of the light as possible would be reflected forward. The arrangement of screens ıally adopted is shown in Fig. 1. The screens D were of polished aluminium and served iefly to prevent radiation from the case B (which became quite hot during the course of the ɔrk) from reaching the instrument F. Screen E, of card, white to the lamp, dull black to the strument, served chiefly to restrict stray laboratory light. The front glass surface of the mp scattered some light and the back surface produced an undesirable image of the filament. irect determinations of the intensities of these radiations, with a Moll thermopile, showed em to be small. The effect of stray radiation was tested using the Ångström instruments 100 cm from the lamp, taking observations both with the front cap G in position and with absent. The first method restricts the front aperture to about half that of the second. he mean difference over all four instruments was just one part in a thousand and no attempt as made to correct the work for this small error.

(*b*) *Ångström instruments*

Four instruments were available : No. 24, standardized at Uppsala in 1924, No. 100A andardized at Uppsala in 1949, both on the Ångström scale, No. 100B standardized at Jppsala in 1950 on the Smithsonian scale, apparently by indirect reference to a substandard eld at Potsdam (see below), and No. 116, not standardized. Most of the work was done ithout the front caps in position, enabling the instruments to be brought much closer to ıe lamp, with the consequent use of higher intensities than would otherwise have been ossible.

Prior to 1950 the constant for each instrument by standardization at Uppsala was given s one figure, it being stated that the zero of the galvanometer referred to the position when oth strips were illuminated. The 1950 certificate for 100B gave a separate value for each rip, the zero being for both strips shaded from the illumination. The practice at Kew for ɔme years has been to use the galvanometer zero corresponding to both strips shaded, and ɔ take the average of the currents through each strip. This mean value is then squared and ıultiplied by the factor supplied (k) to obtain the radiation intensity. Throughout the work escribed this method has been used. For the difference in currents obtained (a few parts ı a hundred only) this approach leads to no error as great as one part in a thousand. The Jppsala factor quoted for 100B is the mean of the two figures given.

The milliammeter used was a rather old instrument, but it was compared at three tempera- ures with a modern instrument calibrated at the National Physical Laboratory in December 950. The nominal accuracy of this calibration was one part in a thousand. Currents used ere of the order of 120 ma and estimation to 1/20 of a division gave a nominal accuracy of eading of $\frac{1}{8}$ per cent, i.e., $\frac{1}{4}$ per cent in radiation intensity. A total of some 100 to 200 readings as obtained with each of the four instruments.

(*c*) *Silver disc instrument*

The instrument used, No. 28, has the old short (20 cm) tube, with front aperture 37 mm n diameter. It was standardized at Washington in 1916. The readings were the means of 0 successive cycles of operation (as against the usual two for solar work), extending over 2 min. The maximum corrected temperature rise of the silver disc in the 100 sec exposure eriod was only about 1°C at the highest radiation intensity used. Occasional sticking of the ercury thread and the inevitable random errors of reading (estimation made to 0.005°C, he thermometer being divided to 0.1°C.), coupled with the variations in output of the lamp, ed to fairly frequent apparent variations in intensity of $\pm$ 2 per cent. After early experiments n procedure the general method adopted was to obtain a silver disc reading, then one (or more) eadings of each of the Ångström instruments, a silver disc reading, and so on. Such a series xtended over two or three days. Including the early experiments there were five such series, iving a total of 200 (i.e. 20 $\times$ 10) corrected readings. All the values obtained from any one nstrument were meaned and a constant (k) for each Ångström instrument evaluated in terms f the Smithsonian (1913) scale, as given by the Kew silver disc instrument. As the Ångström

readings were taken symmetrically, as far as was possible, with regard to those of the silver disc, the effect of the long-term deterioration of the lamp (some parts in a thousand during any one series) was probably only of the order of one part in a thousand.

During the work it was found that if the intensity of radiation appropriate to the position of the actual surface of the silver disc was used, there was inconsistency between the results of observations with this instrument at different distances from the lamp. Two approaches to the determination of the effective position of the surface of the silver disc were made :
(i) Additional readings were taken, three with the disc surface 60 cm away from the filament and six at 90 cm. On the assumption of a point source and an inverse square law the distance between the source and the disc was found. Direct measurement gave a distance 3·1 cm. greater.
(ii) All the work was analysed, using the method of least squares, to minimise the variation of the silver disc-Ångström ratios. A well defined minimum was obtained with the assumption of 3·2 cm as the distance of the effective surface in front of the disc surface.
The second value was taken for the following reasons :
(a) A drawback to the first method is that the distance of the effective surface ahead of the true surface might be expected to be greater the nearer the instrument is to the lamp. In the working on which the general results were based the distance of the disc was 80 to 100 cm with one set at 72 cm.
(b) The second method had the merit of not using the absolute distance of the lamp from the instruments, which was subject to an uncertainty of a few millimetres since the filament consisted of eight lengths of substantial single coiled wire, arranged in a rectangle 2·3 cm × 3·3 cm. The method also used considerably more silver disc readings than the first, and would take care of any similar diaphragm effect in the Ångström instruments.

3. Results

The results are shown in Table I. The July-August series was largely concerned with preliminary work but the results are shown. One or two minor instrumental defects were eliminated as the work progressed. During the early part of the work the join between the thermojunction and the back of one of the strips of 100A was found to be poor and was repaired locally. For this reason 100A cannot now be considered an entirely satisfactory substandard, and the comparisons in which it was involved should be treated with some reserve. In obtaining the mean value of the constants the last three series only have been used, and half the weight has been given to the September series compared with the other two.

The consistency of the results is better than was expected. The work suggests that with refinements, such as an automatic voltage stabiliser, laboratory comparisons between substandard pyrheliometers can be made with an accuracy approaching one part in a thousand, with little difficulty and quite rapidly. Most important, the complication due to sky radiation is reduced to an almost negligible amount. Fig. 2 shows the relation between the particular instruments at Kew, including the work of Watson (1923) and that of Guild (1937b) who both

TABLE I. Laboratory comparisons of Ångström and silver disc instruments

Date	Constants of Ångström No. 24	100A	100B	116	No. of silver disc readings
July-Aug. 1950	15·26 (6)	15·11 (3)	15·29 (8)		2
22-23 Aug. 1950	15·21 (1)	14·91 (1)	15·29 (1)		3
26-27 Sep. 1950	15·39 (2)	14·95 (2)	15·29 (2)	11·31 (2)	4
11-24 Jan. 1951	15·39 (8)	14·94 (2)	15·30 (4)	11·19 (3)	7
12-13 Feb. 1951	15·43 (3)	14·91 (4)	15·31 (3)	11·27 (3)	4
Mean k'	15·41	14·93	15·30	11·25	
Uppsala value (k)	14·99	14·49	15·31	—	
$(k'-k)/k'$ per cent	2·7	2·95	− 0·1	—	

Figures in brackets show number of Ångström observations.

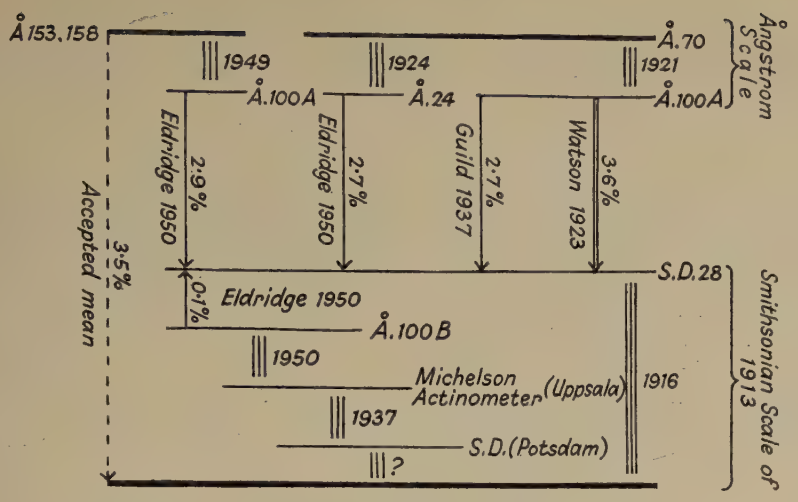

Figure 2. Relation between the Ångström and Smithsonian 1913 scales of radiation. The separations between the horizontal lines indicate the percentage differences between the two scales as they are embodied in the various instruments mentioned in the text. The heavy horizontal lines above and below refer to the absolute instruments at Uppsala and Washington respectively ; the thinner horizontal lines to substandard instruments, the type (and identification number when known) being shown. An identity symbol connecting two horizontal lines indicates a (solar) calibration used to define the scale of a substandard instrument, the date of calibration being shown. Single arrows show the results of laboratory measurements of the percentage differences between the scales of the substandard instruments, the date and authority being shown. The double arrow shows the results of a solar measurement.

used the same silver disc instrument and one of the Ångström instruments used in the present work. The year of the comparison, or calibration, is shown. Ångström 24 has been in continuous use as the Observatory standard for a quarter of a century. It is related to the absolute standard No. 70 which appears to have been superseded at Uppsala before 1949 by Nos. 153 and 158*. The silver disc has had only intermittent use during its 35 years at Kew. However, the figure of 2·7 per cent for the difference between these two instruments agrees with that obtained by Guild (1937b) who used Ångström No. 100A. This suggests consistent standardizations at Uppsala and good stability for both instruments.

The argument through Ångström Nos. 100A and 100B on their recent Uppsala calibrations gives an independent, though less satisfactory check on the relation of the two scales. No. 100B was calibrated there with reference to a Michelson actinometer which had been standardized in 1937 against a silver disc instrument at Potsdam, and since then regularly compared, using the sun as source, with Ångströms 153 and 158*. The difference between the scales is stated to be 3·5 per cent (given on calibration certificate, see also, Ångström 1949) and the agreement between the Kew silver disc and No. 100B only means that this scale difference *through the series of comparisons shown in the figure*, in which some sky-radiation errors must be expected, is confirmed.

4. Conclusions

It is not relevant to the present work to make a detailed study of the merits and defects of the instruments used, but two conclusions may be drawn : firstly that it is clearly most desirable that the geometry of the receiver and the diaphragms of radiometers used for meteorological work should be standardized, and secondly that in the absence of such standardization it is not possible to make valid comparison of different instruments using the sun as a

* Information given in private correspondence by Prof. Ångström.

source, but that such comparison is possible in the laboratory*. The problem of standardization of a solar radiation recording instrument by means of a substandard of different construction will of course remain, but the existence of one cause of inaccuracy is not a valid reason for the perpetuation of another.

Acknowledgements

Thanks are due to Dr. G. D. Robinson for much helpful discussion of the work and to the staff of Kew Observatory for assistance ; also to Mr. R. G. Carter, who helped considerably with the early work. I have to thank the Director of the Meteorological Office for permission to publish this note.

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* A valuable theoretical contribution to the estimation of the additional radiation received from the sky for instruments with either rectangular or circular diaphragms has been made by Bossy and Pastiels (1948). But the application of this work involves detailed knowledge of the intensity and distribution of this radiation, which is not at present practicable as a routine matter.

Correspondence

551.506.1

The Greenwich temperature record

By G. S. Callendar

It was with regret that we learnt from the Astronomer Royal's article in the October 1951 *Journal*, p. 672, that the Greenwich Observatory Meteorological record has terminated. These observations, which include almost a million hourly temperature recordings taken under closely controlled conditions, have continued without a break since 1841, and, as they extend over a longer period than almost any others of comparable accuracy, it seems appropriate to put on record the fluctuations of mean temperature which they reveal.

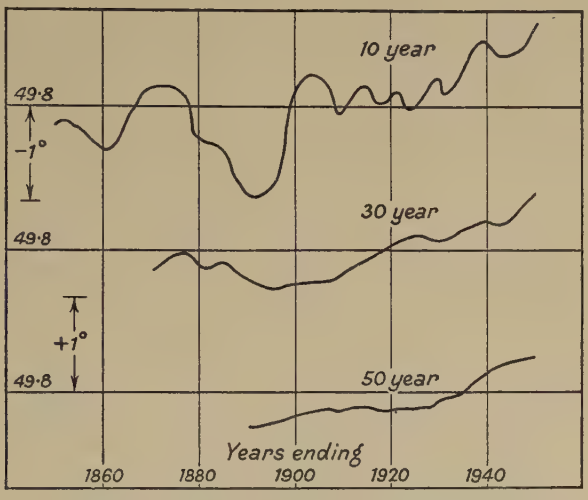

Figure 1. Temperature variations at Greenwich.

The accompanying diagram shows the moving average of annual temperature for several periods of years. The upper curve shows that fluctuations of the ten-year average have amounted to about 2°F during the period, and perhaps the most striking feature of this curve is the low values which occurred about 1890. It is known that similar cool periods had occurred previously about 1815 and 1840 (Manley 1946). More significant, however, than these temporary, and sometimes local, fluctuations is the small upward trend shown by the lower curve of 50-year averages, because it is known that this trend, in varying degree, is representative of a vast area of the north Atlantic region, and may extend over much of the earth's surface (Lysgaard 1949).

Because of the known accuracy of the Greenwich temperatures any definite trend which they reveal is a matter of considerable climatological interest. But, however accurate in themselves, instrumental indications of this kind are very vulnerable to local influences, and although the site of these observations is very favourable, being situated on high ground in Greenwich Park, the conditions at greater distances are by no means so propitious. In fact the general area within a few miles of the park has seen much building activity within the last two or three decades, and, because of the effect such activities can have on local air temperatures, it is necessary to check the trend shown by the Greenwich figures against that found in rural areas nearby. The more so as a glance at the middle curve of 30-year averages shows that the trend is mainly confined to recent decades. This has been done to cover the significant period of the last half-century, with the aid of temperature readings from some reliable rural stations in south-east England. As one of the senior British records Oxford is also included in the comparison.

The result is shown as the change of temperature between the successive 25-year periods 1900 to 1924, and 1925 to 1949, at the following meteorological stations:

Woburn	Agricultural Experimental Station	0·42°F
Wisley	Botanical Gardens	0·63°F
Ventnor	Hospital	0·56°F
Greenwich	Observatory	0·55°F
Oxford	Radcliffe	0·59°F

It is rather surprising to find that the two urban stations show no greater change of temperature than the others, in spite of a large increase in the local population at the former. This increase has been very large over the period at Oxford. In any case the comparison gives great confidence in the Greenwich figures, and indicates that their upward trend is fully significant.

Undoubtedly this fine series would make excellent material for a comprehensive memoir. Perhaps we may hope that some enthusiast will undertake this formidable but valuable task, and thus provide a change from the spate of mathematical papers which are so dreary to the non-specialist.

' Percuil,'
Parsonage Road, Horsham.
18 December 1951

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551.511

The quasi-geostrophic wind approximation

By N. P. Sellick

I have recently been examining the tendencies in the thickness of a layer of air bounded above and below by isobaric surfaces determined to fit simple instantaneous flow patterns. The results obtained have been found to differ materially from the solution offered by Sutcliffe's Eq. 14 (1947) and I was led to examine the approximations on which this equation is based. Sutcliffe proceeds from Eq. 9, a version of the vorticity equation, by ingenious steps which involve the assumption of quasi-geostrophic motion, to his final Eq. 14. The assumptions of quasi-geostrophic motion made are examined below in a model which is intended to represent an instantaneous state of the atmosphere.

Consider a simple pattern of flow in an isobaric surface determined by the velocities

$$u = \frac{2\omega \cos\phi}{Rb^2} + c - a \sin by \; ; \quad v = a \sin b(x - ct)$$

in which a is the velocity of circulation, b determines the dimensions of the cells and c is the velocity of translation. The remaining symbols are traditional and the first term in ϕ is the ' Rossby ' term which satisfies the vorticity equation when treated as a constant.

The vorticity of this wind field is

$$\zeta = ab\{\cos b(x - ct) + \cos by\} \qquad (1)$$

and the divergence is zero. The field of contours to balance the wind field is

$$h = -a^2 \cos b(x - ct) \cos by - \frac{a}{b} l\{\cos b(x - ct) + \cos by\} + h_0 \qquad (2)$$

together with terms in ϕ which become small when differentiated.

Now Sutcliffe, Section 7, argues, on the quasi-geostrophic wind assumption, that

$$\zeta = \frac{1}{l} \nabla_p^2 h \quad \text{approximately.}$$

Performing this operation on the field in my Eq. 2,

$$\frac{1}{l} \nabla_p^2 h = \frac{2a^2 b^2}{l} \cos b(x - ct) \cos by + ab \{ \cos b(x - ct) + \cos by \}$$

ogether with small terms in ϕ, and the validity of the assumption depends on the first term eing negligible in relation to the second. The trigonometrical functions give the second erm an advantage of two to one, hence the ratio of the terms is determined by ab/l or say $0^4\ ab$ in second units. Taking $b = 5 \times 10^{-8}$ to give the cyclone an apparent diameter of ,100 km and $a = 1{\cdot}0 \times 10^3$ giving a maximum circulatory velocity of 10 m/s, ab/l is 0·5 nd the quasi-geostrophic assumption involves errors which can hardly be termed small.

Further, in Section 9, Sutcliffe invokes the quasi-geostrophic relation to show that

$$-\frac{1}{l}\left(\frac{\partial}{\partial t}\right)_p \nabla_p^2 h' = (V_0 . \nabla_p) \zeta - (V . \nabla_p) \zeta_0 \quad \text{approximately.}$$

Now h' can readily be found for a layer by assuming that my Eq. 2 applies to the bottom f the layer and the same equation, retarded by 60°, applies to the top of the layer. The ickness pattern is then given by

$$h' = \left(a^2 \cos by + \frac{la}{b}\right) \sin (bx + 30°) \qquad (3)$$

ence

$$-\frac{1}{l}\left(\frac{\partial}{\partial t}\right)_p \nabla_p^2 h' = -bc\left(\frac{2a^2 b^2}{l} \cos by + ab\right) \cos (bx + 30°)$$

ut the solution of the right-hand side of Sutcliffe's equation is

$$-ab^2\left(\frac{2\omega \cos \phi}{Rb^2} + c - 2a \sin by\right) \cos (bx + 30°)$$

Comparing these two, the first term in ϕ is small and the terms in c are identical but he terms in $\sin by$ and $\cos by$ are very different. If the approximation is valid they must e small in relation to $ab^2 c$. The terms are

$$ab^2 c \ : \ \frac{2a^2 b^3 c}{l} \ : \ 2a^2 b^2$$

$$\text{or} \quad 1 \ : \ 2ab/l \ : \ 2a/c$$

nd, as they are of the same order, the approximation again fails.

.O. Box 66,
auseway,
. Rhodesia.
7 December 1951

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Reply

By R. C. Sutcliffe

Mr. Sellick's second point although interesting has, I think, little bearing on my problem. n defining his model he defines the thermal field by virtue of the hydrostatic equation but here is no reason to suppose that such a thermal field could be maintained thermodynamically.

It would require a ' heating function,' imposed from outside, to offset the advective changes naturally, the order of magnitude would be the same as the advective change which alon is included in my simple formula.

His first point is however important and I welcome the opportunity to say again wha I think I have said more than once in my papers, that the approximations render my theor invalid for small-scale synoptic features, and a wave length of 1000 km is of course sma on the synoptic scale. Nevertheless there is good reason to suppose that even the smalle systems, provided that they can be mapped on synoptic charts and that they are largel controlled by the earth's rotation, are similar in structure and evolution to the larger feature and that considerations of the geometry of the thermal and pressure patterns derived fo larger-scale systems may be more helpful in practice than might be expected from theory.

One may point out that in Mr. Sellick's model the geostrophic-vorticity approximatio has the same degree of validity as the geostrophic-wind approximation itself. This is pretty good generalization for the atmosphere as a whole and if one could handle dynamica developments with such broad accuracy we should be making very good progress. Th main dynamical difficulties go even deeper than Mr. Sellick's analysis, to the effects of stability to adiabatic and non-adiabatic temperature changes and to the release of latent heat. S far there is for the forecaster no simple quantitative expression for the dynamical effects o these processes and much ' judgment ' must supplement partial theory.

Meteorological Office,
Dunstable.
18 January 1952

551.524.7 : 551.501.4

On the relation between the standard deviations of temperature at constant pressur and at constant height

By M. W. Chiplonkar and M. L. Nandgaonkar

The following formula connecting the standard deviation of temperature at constan pressure (${}_p\sigma_T$) with that at constant height (${}_h\sigma_T$) was first suggested by Miss N. Carruther and its formal proof was supplied by A. F. Crossley (1950).

$$ {}_p\sigma_T{}^2 = {}_h\sigma_T{}^2 - 2\frac{\partial T}{\partial p}\,{}_h r_{pT}\,{}_h\sigma_p\,{}_h\sigma_T + \left(\frac{\partial T}{\partial p}\right)^2 {}_h\sigma_p{}^2 . \qquad (1) $$

In order to test the validity of this relation the observed values of ${}_p\sigma_T$ at a number of levels up to 20 g km in the upper atmosphere over a few typical tropical stations (Agra, Poona, Hyderabad and Batavia) were compared with those calculated from the above formula (see Table I) using Dines meteorograph data (Chiplonkar 1936). The results obtained with more recent radio-sonde data do not differ significantly from those given.

It will be seen from this table that on the whole the observed and calculated values of ${}_p\sigma_T$ agree fairly well in the troposphere. But near and above the tropopause large differences, as much as 1·8°C, are found. Obviously the calculated values of ${}_p\sigma_T$ are generally smaller than those of ${}_h\sigma_T$ in the troposphere. They are equal or there is just a tendency for ${}_p\sigma_T$ to exceed ${}_h\sigma_T$ in the stratosphere, the largest difference being only about 0·5°C. However, no such systematic relation exists between the observed ${}_p\sigma_T$ and ${}_h\sigma_T$ taken individually.

We wish, next, to draw attention to the following considerations regarding the formula (1) and its derivation as given by Crossley.

An inspection of this formula shows that it is nothing but the well-known statistical formula used for obtaining the standard deviation of the sum or difference of two variables, viz.

$$ \sigma^2 = \sigma_1{}^2 + 2r_{12}\,\sigma_1\,\sigma_2 + \sigma_2{}^2 \qquad (2) $$

where r_{12} is the correlation coefficient between the two variables and σ_1 and σ_2 their respective standard deviations (Yule and Kendall 1950). If, therefore, the temperature at *constant*

TABLE I

Agra (27°10′N, 77°58′E). Period 1926-1931.

	(a) Dec.-Feb.				(b) Sept.-Oct.			
h km)	p (mb)	$h\sigma_T$ (°C)	$p\sigma_T$ (observed)	$p\sigma_T$ (calculated)	p (mb)	$h\sigma_T$ (°C)	$p\sigma_T$ (observed)	$p\sigma_T$ (calculated)
2	797	3·1	2·9	3·0	795	1·7	1·9	1·7
4	617	3·6	3·1	3·4	623	2·9	2·8	2·9
6	475	3·3	3·0	3·0	483	4·4	3·7	4·3
8	359	3·8	3·4	3·3	370	3·8	3·9	3·5
10	267	4·7	4·8	4·4	279	3·6	3·3	3·1
12	196	4·9	4·6	4·7	207	3·4	3·2	2·9
14	141	4·5	4·3	4·5	151	3·0	3·5	2·5
16	101	4·0	4·1	4·0	107	3·9	3·8	3·9
18	72	4·4	4·2	4·4	75	5·2	4·4	5·2
20	51	5·7	4·1	5·7	53	5·5	4·4	5·6
Poona (18°31′N, 73°52′E.) Period 1928-31					Hyderabad (17°22′N, 78°30′E.)			
2	799	2·8	2·9	2·7	797	1·9	1·9	1·9
4	623	2·1	1·9	2·0	623	1·7	1·6	1·7
6	481	2·2	2·2	2·1	483	1·5	1·9	1·5
8	367	2·9	2·9	2·8	370	2·2	2·0	2·1
10	276	3·3	2·9	3·2	278	2·4	2·2	2·2
12	202	4·1	3·3	3·7	207	2·7	2·6	2·4
14	146	4·9	5·1	4·9	148	3·5	2·9	3·1
16	103	5·6	5·1	5·3	105	4·5	3·7	4·0
18	73	5·7	4·0	5·8	74	6·6	7·2	6·5
20	52	5·9	5·7	6·1				
Batavia* (6°10′S, 106°48′E.) Period 1910-1915								
2	806	1·1	1·2	1·1				
4	634	1·3	1·4	1·3				
6	495	1·3	1·6	1·4				
8	379	2·0	1·6	2·1				
10	289	2·6	2·7	2·6				
12	215	4·0	3·5	3·8				
14	156	4·5	3·7	4·0				

* Bemmelen (1916). Insufficient number of observations during the period Sept.-Oct. for corresponding table to be prepared.

pressure could be expressed as a sum or difference of two temperatures at *constant height* the desired relation would straightway be written down. Thus, if as a first approximation a linear relationship between p and h is assumed, we may put in the form of partial differentials

$$\delta T_p = \delta T_h - \left(\frac{\partial T}{\partial p}\right)_h \delta p_h \qquad . \quad . \quad . \quad . \quad . \quad (3)$$

Application of Eq. (2) then immediately gives the required formula (1). Note that here $(\partial T/\partial p)_h$ means $(\partial T_h/\partial p_h)$, the partial differential coefficient of temperature at constant height with pressure at constant height. It may, however, be pointed out that Crossley puts it first as $\delta T_p = \delta T_h - (\partial T_p/\partial p)\,\delta p$ and while averaging changes $(\partial T_p/\partial p)$ to $(\partial T/\partial p)$ which naturally is *not* at constant height but at constant *pressure*.

If on the other hand starting from the more exact and standard relation $p = p_0 \exp -\frac{g}{R}\frac{h - h_0}{T_m}$ we obtain the necessary partial differentials and substitute them in the Eq. (3), no simple and relevant relation is obtained; thus showing the ultimate inadequacy of the differential relationship (3) itself.

Hence, the proposed formula is not sufficiently accurate and the agreement between the calculated and the observed values of $_p\sigma_T$ *shown above is not close enough.*

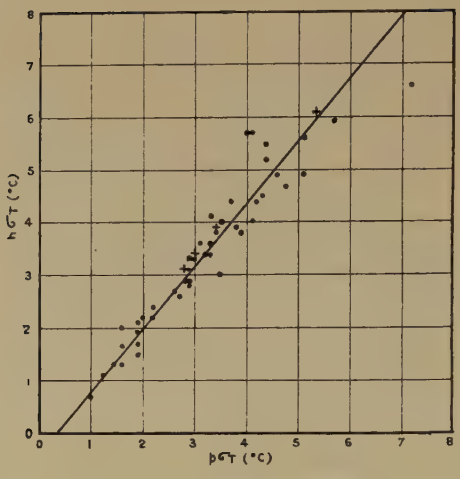

Figure 1. The relation between the observed standard deviations of temperatures at constant height and at constant pressure. Crosses indicate the Larkhill values.

Alternatively, the observed values of ${}_h\sigma_T$ (given in column 3 of Table I) were plotted against those of ${}_p\sigma_T$ (given in column 4) as shown in Fig. 1. Larkhill values obtained by Crossley are also marked here. A mean straight line drawn through these points gave the relation ${}_p\sigma_T = 0{\cdot}86\ {}_h\sigma_T + 0{\cdot}30$; while the method of least squares applied only to the tropical values gave ${}_p\sigma_T = 0{\cdot}877\ {}_h\sigma_T + 0{\cdot}175$.

We are extremely grateful to the Director General of Observatories, India, for allowing us the use of the library facilities of the department.

Heracles Home, Poona;
Meteorological Office, Poona.
25 January 1952.

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Yule, G. U. and Kendall, M. G.	1950	*An introduction to the theory of statistics*, 14th Edn., London (Griffin), p. 326.

Reply

By A. F. Crossley

Messrs. Chiplonkar and Nandgaonkar try to show that the relationship under discussion is incorrect. I am unable to agree with their arguments. Their Eq. (3) does not follow, as they state, from the assumption of ' a linear relationship between p and h '; moreover the exponential relationship quoted later is irrelevant if only because it does not contain the temperature in an appropriate form. Further, their Eq. (3), which actually derives from a linear relationship (to the first order) between T_p and p, is formally correct only if it contains T_p (instead of T) in the differential coefficient on the right. It is only after taking mean values, as I made clear, that $\partial T_p/\partial p$ can be replaced by $\partial T/\partial p$, and even then it was implied that this step could be justified only if the fluctuations of lapse rate (from one occasion to another)

were distributed about the mean with a reasonably small scatter, although the details of this argument were omitted. On this account I had not expected the formula to be strictly applicable in the vicinity of the mean tropopause, and limited my Larkhill data to 300 mb. Within the stratosphere, theoretical equality of the two standard deviations follows directly from the assumption of zero lapse rate, independently of any formula. Consequently the large differences given by Chiplonkar and Nandgaonkar for Agra at 20 km (1·6°C in Dec.-Feb. and 1·1°C in Sept.-Oct.) and for Poona at 18 km (1·7°C in Dec.-Feb.), the mean tropopause being in the neighbourhood of 16 or 17 km, require explanation. They perhaps arise from the inadequate observations at these high levels, and may not be truly representative.

Meteorological Office,
London.
5 February 1952

551.311.18 : 551.509.33

Foreshadowing the severity of the iceberg season off Newfoundland

By I. I. Schell

In a paper appearing some time earlier in the *Journal* (Walker 1947) attention was called to an apparent breakdown of the relationships that have figured in the foreshadowing of the severity of the iceberg season off Newfoundland (Smith 1931). It would appear, however, that on examining the factors involved, Walker (*ibid.*) did not consider quite the same combination of months for the pressures used as was done earlier. What may be more significant, while some of the factors have indeed materially weakened in their apparent control of the ice, others seem to have not and may in fact have strengthened in the more recent period. This raises the question whether in the combinations initially determined by Smith (*ibid.*) and also Groissmayr (1939) there may not have occurred enough mutual compensation in the relationships with the ice to offset the change Walker's analysis suggests.

Employing the full body of data now available, coefficients of correlation of the ice count with the pressure differences between Belle Isle and Ivigtut during December to March and between Bergen and Stykkisholm during October to January, respectively, were computed for the period ending with 1948/9, as a whole and for the first and second halves of this period separately (see Table I). Shown in the same table are correlation coefficients of ice count with the preceding December to March temperatures of St. John's, Bermuda and Uppsala which, in a somewhat different form, were previously indicated by Groissmayr (*ibid.*) to reflect the ice off Newfoundland the following spring and early summer. From the coefficients in Table I, it appears that while the relation of the October to January, Bergen

TABLE I. Correlation coefficients of the iceberg count off Newfoundland in spring and early summer with the preceding average October to January, Bergen to Stykkisholm, and December to March, Belle Isle to Ivigtut, pressure differences and with December to March St. John's, Bermuda, and Uppsala temperatures

	Pressure differences		Temperatures		
	Belle Isle-* Ivigtut	Bergen-Stykkisholm	St. John's	Bermuda	Uppsala
	r	r	r	r	r
1879/80-1948/49	0·53	0·30	— 0·49	0·40	0·55
1879/80-1913/14	0·44	0·47	— 0·60	0·41	0·54
1914/15-1948/49	0·58	0·08	— 0·39	0·36	0·58

* Data for Belle Isle prior to 1882 were not available to the author. The periods examined here are 1882/3-1948/9, 1882/3-1915/6, 1916/7-1948/9, respectively.

to Stykkisholm, pressure gradient with the ice the following season has indeed virtually ceased, probably on account of the northerly shift of the Icelandic Low in recent decades, the relation of the December to March, Belle Isle to Ivigtut, pressure difference with the ice has somewhat strengthened in the second half of the period. Similarly, while there has been some loss in association of the ice with the preceding St. John's and Bermuda temperatures in the second half of the long period examined, this loss was to some extent compensated by the increased association with Uppsala temperatures in the same period. Taking the December to March, Belle Isle to Ivigtut, pressure gradient and St. John's, Bermuda, Uppsala temperatures together, the overall association with the ice would appear to have been maintained from the first half to the second half of the period.

As a further test of the relationships between the ice off Newfoundland and the preceding Belle Isle to Ivigtut pressure differences and St. John's, Bermuda, and Uppsala temperatures, the values of the ice departures covering the recent 25-year period, 1927-1951, were computed with the aid of the formula:

$$\Delta \text{ ice} = 0{\cdot}12\ \Delta \text{ press. diff. (B.I.-I.)} - 0{\cdot}31\ \Delta \text{ temp. St. John's} + 0{\cdot}19\ \Delta \text{ temp. Bermuda} + 0{\cdot}17\ \Delta \text{ temp. Uppsala}$$

based as earlier on data to 1926 inclusive and are shown in Table II with the observed departures.

TABLE II. Computed and observed departure (Δ) from the long-term average (1880-1926) in the iceberg severity off Newfoundland 1927-1951, on scale of 0 to 10

Year	Computed	Observed	Year	Computed	Observed
1927	− 0·1	0·0	1940	− 2·7*	− 4·7
8	0·3	0·8	1	− 1·5*	− 4·7
9	− 0·2	4·2	2	− 2·4*	− 2·4
1930	2·0*	0·6	3	0·8	2·4
1	− 1·8*	− 2·8	4	− 0·3	2·0
2	0·5	0·8	5	− 0·8	3·9
3	− 0·1	− 1·1	6	− 0·4	0·1
4	1·4*	1·0	7	− 1·5*	− 2·0
5	0·8	2·7	8	0·4	1·1
6	− 2·0*	− 2·8	9	0·4	− 2·1
7	0·4	0·6			
8	1·1*	1·8	1950	1·1*	0·4
9	1·1*	1·8	1	− 2·0*	− 4·7

* Values in excess of ± 1·0 on the scale of 0-10, where 4·8 = average iceberg count (440) on a non-linear scale 0 to 10.

Of the 25 cases involved, non-agreement as to sign between the computed and observed deviations occurred in five, and of the 12 cases with a computed deviation in excess of ± 1·0 disagreement as to sign occurred in none. The coefficient of correlation between the computed and observed deviations for the 25-year period as a whole is $r = 0{\cdot}80$.

It may be added that while Walker's own suggestion about the pressure distribution over the Canadian Archipelago, etc., having important bearing on the ice off Newfoundland is in line with the fact that before descending southward to Newfoundland, the icebergs abound in the western half of Baffin Bay, the use of pressures from Pt. Barrow, the only station available to him, fails to show the proper relation.

The coefficient of correlation of the October to November pressures from that station with the following spring and early summer ice off Newfoundland using the additional data since become available, is $r = -0{\cdot}09$ (1899/1900-1948/9) instead of $r = -0{\cdot}74$ (1921/2-1938/9) originally determined. Also the assumption that lower than normal pressures at Pt. Barrow during October to November mean stronger winds from the west is not borne out by the fact that the prevailing wind at that station during the same months is from the northeast.

Out of the 36 years of observation to 1948, the wind was from the west only in the year 1935, nd one must await more comprehensive data from that general area before the interesting ossibility suggested by Walker can be further explored.

Woods Hole Oceanographic Institution,
Mass., U.S.A.
29 November 1951

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551.574.7

THE ICING OF AIRCRAFT AT TEMPERATURES BELOW —41°C

By R. FRITH

Mr. B. J. Mason (1952) refers to a report of ice formation on the wing of a Mosquito aircraft in a 'propeller' condensation trail, at a level where the temperature was below —41°C. He rightly considers that some explanation is called for and presents one based upon the theory expounded in his paper.

Mr. Mason is too uncritical. There are three suspicious points in the icing report. Firstly, that the trail is dense and persistent. There simply is not enough water vapour at these levels to form a dense trail and a propeller-tip trail could not be persistent in the turbulent wake of the aircraft. Secondly, that the ice was observed to spread back over the surface of the wing. Mr. Mason realizes this difficulty; but states, rather oddly, that there is no real evidence that ice did continue to form. Thirdly, that the trail was most dense at the centre. This remark is decidedly difficult to apply to a propeller-tip trail.

A less obvious difficulty is that the surface of the wing is very much warmer than the air, owing to dynamic heating. There could be no perceptible ice formation from a spiralling propeller-tip trail on such a surface.

In fact, the ice formation occurred, not in a propeller-tip trail, but in an exhaust trail. I suspect that a 'propeller trail' was merely the pilot's term for a condensation trail; but if there were a propeller-tip trail then the dense part at the centre would be the exhaust trail.

Ice formation on the wing of a Mosquito aircraft in exhaust trails is not rare. I have seen it a number of times. When a Mosquito aircraft is flying with a high angle of attack (e.g., when climbing hard at 400 mb) the exhaust stream is divided by the wing and if condensation trails are forming they can clearly be seen over the upper surface of the wing. In these circumstances, ice formation generally occurs on the leading edge of the wing, behind the engine. This ice spreads back, quite perceptibly, over the surface of the wing.

There is, of course, no difficulty in explaining ice formation in an exhaust trail a few inches from the exhaust outlet.

Meteorological Office,
Harrow.
14 February 1952

REFERENCE

Mason, B. J. 1952 *Quart. J. R. Met. Soc.*, **78**, p. 26.

Reply

By B. J. Mason

It is quite possible that Dr. R. Frith is right in saying that the pilot was mistaken in reporting what was actually an exhaust trail as a propeller trail, but we cannot be certain.

In the last paragraph of my paper I wished only to point out that if, in fact, droplets were produced by expansion of the air in the vortices produced by the tips of the propeller blades formation of ice on the leading edge of the wing at air temperatures below — 44°C was not necessarily inconsistent with my laboratory observations in which freezing of 5 μ droplets occurred very rapidly below — 41°C.

Of course, if the trails were formed by the exhaust no theoretical difficulty arises.

Department of Meteorology,
Imperial College,
London.
14 March 1952

551.515.8

A study of cold fronts over the British Isles

By H. W. SANSOM

(Read 21 November 1951 (presented by Dr. R. S. Scorer). See *Q.J.*, **77**, p. 96)

Discussion

D. G. Harley : I would like to criticize the analysis of the tephigram used as an example of the 'katafront,' as the air indicated to be above the frontal surface in the post-frontal ascent had not only lower temperature but very much lower relative humidity than the warm-sector air, making it appear to be subsided cold air, not warm air. I would also question the method used to distinguish between the air masses, as this has a direct bearing on the calculated frontal slopes. Using pseudo-potential wet-bulb temperature which is the most reliable method of analysis, the frontal surface would appear to be in the upper parts of the ascent, while the inversion considered in the paper as due to the frontal surface appears to be due to subsidence in the cold air.

J. S. Sawyer : I was interested in Mr. Harley's remarks because it appeared to me also that there is some uncertainty in Dr. Sansom's work regarding the identification of the boundary of the true warm air mass in the katafront. Perhaps this difficulty arises because no sloping air-mass boundary is present. It may be best to study the katafront without endeavouring to fit it closely into the Norwegian air-mass model. At a warm front we have two masses of air with differing water contents and it is possible to identify these as air masses of differing origin. This is also true at the anafront, but at the katafront, temperature is the only clear distinguishing property. In seeking an understanding of the katafront it may therefore be best to forget the Norwegian theory, and seek a dynamical explanation as to why a sloping discontinuity of temperature should form and persist.

A. G. Matthewman : Dr. Sansom is to be congratulated on a very interesting paper. There is no doubt of the practical importance of the vertical motion above a cold front. I was particularly interested in Dr. Sansom's use of the associated hodographs. But I think the form and interpretation of the hodographs are more complex and subtle than appears.

Dr. Sansom states that his Eq. (4) for the vertical velocity, $w = (c - u) \tan \theta$, is based on a steady two-dimensional flow, so cannot be used to obtain accurate values of the vertical currents at a front. He also states that the variation of the wind component parallel to the front may contribute considerably to the vertical motion.

It seems to me that his Eq. (4) is necessarily true for any material surface, whether plane or curved, accelerated or moving steadily, turning or changing slope, or moving parallel to itself; it is just a kinematic expression of the fact that the surface *is* a material surface. But strictly it is necessary that c, u, and θ should all be measured at the same point of the surface. Friction and variations in the component of wind tangential to the surface are irrelevant as far as Eq. (4) is concerned.

If then the more or less distinct boundaries of a frontal zone are truly material surfaces, which isn't certain, Eq. (4) should hold exactly. The practical difficulties however lie in determining the orientation, slope and speed of the boundaries at the levels where these boundaries occur on the vertical sounding, and also to some extent on the mode of smoothing, since all the measurements refer to smoothed values over finite intervals. But it should be stressed that the value of w computed from such a set of measurements only applies to the point which the values refer to, and cannot be safely extrapolated much up or down, though extreme cases of change of normal component may be suggestive. Thus it is not remarkable that Eq. (4) applied to the averages in Fig. 2, gives an improbable estimate of w though of right sign.

In an investigation of cloud near fronts of any type, I found that the cloud, and the vertical velocities which I calculated in some cases, by no means supported the idea of solid ascent or descent above the warm boundary of a frontal zone. The calculated vertical velocities were in reasonable agreement with the cloud reported, but both showed sometimes great

variability at a place over five to ten hours. Moreover there was usually more than one front zone involved, and two simultaneous zones may have quite different slopes, orientation and speeds.

I examined a sample of nine well-marked cold fronts, simple according to the surface analysis, during January to March 1950, taking radiosonde observations of wind, temperature and humidity just before and just after the passage of the surface cold front. From the 1 observations behind the fronts, I found, according to the characteristics stated in Section (4 of the paper, 10 katafronts (one or two only weakly), one doubtful anafront and one intermediate. I then deliberately chose an example in May 1949 of a very rainy cold front. The hodograph showed : (i) an increasing normal component from 900 mb to 600 mb, although the speed was less than the normal speed of the surface cold front up to about 670 mb, then (ii) a decreasing normal component to 500 mb to a value 15 kt less than the normal speed of the surface cold front, then (iii) an increasing normal component to 25 kt, more than the normal speed of the surface front at 250 mb.

My impression from these and other soundings is that the hodographs are generally more complex than the average characteristics suggested by Fig. 2 and that in most cases the wind sounding requires careful interpretation; in a number of cases I suspect the interpretation as regards vertical motion by inference from the motion of the surface cold front will be inconclusive.

M. K. Miles : I should like to take the opportunity to say how clearly and readably Dr. Sansom has described and presented a complex mass of important data. Like the two opening speakers I doubt whether the air aloft behind the katafronts should be described as warm air. If it were, then after subsidence it should be much warmer than it is and very unlikely to be so dry.

If we wish to retain our notions of clear-cut air-mass boundaries until we can think diffusely more clearly, then I suggest the following picture for the structure of katafronts. The sloping surface for which values of $\tan\theta$ are given in Table VI is between subsided and unsubsided cold air, and the original warm-cold boundary (carried ahead by the forward shear) is a nearly vertical zone, become diffuse by the subsidence of the cold air, and often only recognizable by the strong humidity gradient.

The occurrence, with many katafronts, of a narrow belt of prefrontal rainfall suggests that the warm air is forced upwards over a limited region by the forward-running subsiding cold air.

Dr. A. E. Slater : Cold fronts are used by sailplane pilots only when they can fly in clear air in advance of the front. But over the British Isles the main cold front of a depression is usually preceded by low cloud occupying the warm sector, and a pilot ascending into this cloud would not be able to see the position of the front.

It has always been something of a mystery why sailplane pilots find the up-currents at a cold front so much stronger than can be accounted for by mechanical lifting. But since their flights are made in clear air, and in daylight, the presence of convection currents (thermals) might well be the explanation. Dr. Sansom, in quoting Rainey's theory that the convection currents are localized into a narrow zone just ahead of the advancing cold air, says (p. 106) ' no explanation has been offered of why or how this is brought about.' But there seems an obvious explanation in the fact that, when convection is active over land, a hot, unstable layer of air is present close to the ground. This layer is reluctant to rise of its own accord and usually needs a push to start it on its upward journey; the arrival of the front would thus trigger it off all along the line.

A. J. Whiten : In the first place, I wonder whether Dr. Scorer can give me a few hints to assist one who, like myself, likes to give an impression of some knowledge, by examining the sky, to determine in advance whether an approaching disturbance is in the nature of an anafront or a katafront ?

We are told that a cold front tends to change from anabatic to katabatic character, near the point of occlusion. As most depressions are in the process of occlusion during the greater part of their lifetime, the front would change from a katafront to the south, to an anafront below the point of occlusion, and an occlusion to the north. At the southern end, the east-west component of alignment would be much greater than further to north, and though the greater speed of this anabatic sector would tend to straighten out the whole front, it seems to me

hat there would be a ' sideways ' movement in this part of the front. Is there any evidence hat this type of movement exists ? and if so, how does it effect the up-thrusting of the warm ir ?

Dr. R. C. Sutcliffe : We must congratulate Dr. Sansom on putting together some ew and striking statistics. The ideas and the terminology are not new, they are not claimed o be new, but the distinction between the anafront and the katafront is very important and Dr. Sansom has done us a good service by emphasizing the point so clearly. We may criticize is cross-sectional analyses but it is largely a matter of convention where one places the frontal surface in a frontal zone. The important point is that cold fronts have these distinct properties nd since the cases were taken from published *Daily Weather Reports* with analyses made t the Central Forecasting Office we may take it that they are cold fronts as ordinarily under-stood; they are not Dr. Sansom's own selection to illustrate a preconceived thesis.

Perhaps the most difficult thing is to understand how a steeply sloping anafront should flatten out so much in spite of over-running and become a katafront sloping at 1 : 300. One might imagine that over-running would tend to cause a front to topple over forwards and, although the argument is naive it is no more naive than the argument that the air subsides because it runs down the frontal surface. We are badly in need of a dynamical treatment of the behaviour of frontal systems. It is indeed remarkable that after more than 30 years of frontology we have no three-dimensional theory of frontal circulations, nothing more than a few bits and pieces of dubious dynamical and kinematical argument.

My own view is that the frontal boundary surface is something of a convention and that the behaviour depends on the structure in three-dimensions of a broad baroclinic zone. For some reason, probably not dependent on the precise slope or other characteristics of the ' frontal surface,' the region behind the cold front trough becomes a subsidence region. The frontal surface then degenerates and a new subsidence inversion is formed within the cold air at a lower level. At the same time there is often convergence ahead of the cold front and in suitable conditions there may be considerable pre-cold-front rain.

It may sometimes be legitimate to regard the subsidence inversion as virtually continuous with the old frontal surface but, unless wet-bulb potential temperatures are in agreement, the practice is liable to be misleading.

However that may be, Dr. Sansom's statistics throw up points which were, to me at least, unexpected, especially the remarkable seasonal variations, and synoptic meteorologists will be prompted to examine further cases critically.

R. Murray (*communicated*) : Dr. Sansom quotes as a good example of the transition with time of an anafront to a katafront the case of the cold front of 7 to 8 April 1950. The Stornoway upper-air sounding at 15 hr on 7 April shows that, in the neighbourhood of Stornoway, the cold front is of an anafront type. Also, undoubtedly, the Downham Market sounding at 09 hr on 8 April indicates that, in the neighbourhood of Downham Market, the front is of a katafront type. However, it does not necessarily follow that the front as a whole has changed from an anafront to a katafront owing to the difficulty involved in deciding the location at 09 hr on 8 April of that part of the front which was near Stornoway at 15 hr on 7 April. If a section of the front is identified by its distance from the low-pressure centre then the part near Stornoway at 15 hr on 7 April must have been somewhere in the Norwegian Sea by 09 hr on 8 April; whilst the Downham Market section of the front at 09 hr on 8 April was probably somewhere SW. of the British Isles at 15 hr on 7 April. The indirect evidence of the synoptic charts strongly suggests that the section of the front over the Norwegian Sea and affecting western Norway at 09 hr on 8 April was still an active anafront; also, that the part of the front SW. of the British Isles at 15 hr on 7 April was already a weak katafront.

Whilst agreeing with Dr. Sansom that a cold front must end its life as a katafront, I think that at any instant a cold front will often show anafront features over one segment merging into katafront features over another segment. However, it is not possible to generalize that one particular section of a front (such as that near the low-pressure centre) is normally an anafront or a katafront in character, since the flow patterns aloft will surely determine where the general up-and-down air motion takes place and, therefore, the character of the frontal segment.

Dr. R. S. Scorer (*in reply*) : Messrs. Harley, Matthewman, and Miles may with considerable justification criticize the detail of Dr. Sansom's analysis in many cases, but we

must bear in mind the purpose of the analysis. As Mr. Sawyer points out, its purpose need not be to fit each situation to the Norwegian model. Perhaps we should invert Dr. Sansom's work and start with definitions of katafronts and anafronts. Then he has shown that most of the fronts he studied could be called by one of the names, and that generally most or all of the characteristics in the definition were to be found together. The terminology can therefore be made use of; for instance, when a clear-cut katafront is encountered, it is economical to call it by that name, or if it is clearly changing from the ana- to the kata-type it would be useful for central forecast offices to say so in their bulletins.

Dr. Sansom describes and classifies phenomena, and does not argue how they should be fitted to the Norwegian model, and so many of the criticisms of the analysis are irrelevant to the purpose of the study. This cannot be said of Mr. Murray's remarks, however; though it is fair to point out that the author concluded from the study of many fronts that they generally changed from the ana- to kata-type, and to criticize the instance chosen for illustration is to criticize the choice, not the conclusion it is intended to illustrate. The choice was very difficult because fronts generally cross the British Isles in too short a time for the change to be obvious.

The formula discussed by Mr. Matthewman applies theoretically to a material point on the frontal surface, but the author had doubts about it because in practice it could only be applied near the surface.

The simplest advice I can give to Mr. Whiten is that the saying ' quick come, soon past ' applies to a katafront, the opposite to an anafront. The tail end of the cold front of a depression is rather outside the scope of the paper, but I think that the sideways component can probably be regarded as superposed on the rest of the motion without substantially affecting discussion of it from this point of view.

(*Communicated later*) : I am unable to contribute to the general dynamical theory of fronts that Dr. Sutcliffe asks for but perhaps I may throw out an idea that can help us in picturing the processes that go on. This idea is the *advection principle* (Scorer, *Q.J.*, p. 76, 1952), which states that meteorological ' goings-on ' are not transmitted from place to place but are carried there by the air itself. They are more like smoke than sound waves. Thus a front is a place where cold air is moving underneath warm air, but the vertical motion seems to be confined to the region of the front. If the cold air were a solid wedge shovelling up the warm air then gravity would level out the warm air in the warm sector; this does not happen, even though the boundary of the warm air is the same whether the cold air is air or solid. The explanation is that the origin of the motion is different. Atmospheric motion is produced by gravity acting on air that has been non-uniformly heated. The wedge picture thinks of warm air that is set in motion by the movement of its boundaries. Since the boundaries are fixed no waves of meteorological importance occur which can transmit a disturbance from one part of the atmosphere to another.

Meteorology is not just a branch of hydrodynamics that can be thought of by analogies from other branches. Such terms as ' forced ascent ' and ' up-thrusting ' must be abandoned and terminology must be taken from aerodynamics and hydraulics with the greatest care, for in those subjects we study fluids that are pushed about. It is hard to think clearly about the atmosphere in a world full of fans, taps, spoons, and water waves, inhabited by beings that breathe, swim, and fly. Our very life goes on within a moving boundary and whether by sound waves or gesticulation it is by moving boundaries that we convey our ideas to others. Perhaps before long we shall be talking about ' troughs of low pressure ', ' ana-troughs ' and ' kata-troughs.'

Dr. H. W. Sansom (*communicated*) : Owing to my absence from Nairobi on temporary duty, I regret that I must reply without reference to my original paper.

In a number of the katafront cross-sections examined the true air-mass boundary differed markedly from the apparent boundary indicated by the subsidence inversion. If the structure proposed by Mr. Miles is correct, then Dr. Sutcliffe's problem of how a steeply sloping anafront becomes a katafront with a gentle slope is partly solved. The original steep anafront boundary may have become much steeper and quite diffuse, while the newly-formed subsidence inversion behind this boundary has given rise to a new cold front, sloping at about 1 : 300, within the original cold air.

The Norwegian frontal model was an ideal, and for some time we have found that a

large number of fronts, and especially cold fronts, do not quite fit the model. I have sought to revise the model in the light of experience, and in so doing have naturally made it a little more complex. It is, however, still an ideal model, and, as Mr. Matthewman and others have shown, there are many cold fronts which still may not quite fit the picture.

551.510.53 : 551.524.7 : 551.508.822

The apparent diurnal temperature variation in the lower stratosphere

By R. H. KAY

(Read 21 November 1951. See *Q.J.*, **77**, p. 427)

Discussion

Dr. F. J. Scrase : Unless one has an absolute standard of comparison such as the measurement of the velocity of sound, suggested by Dr. Kay, it is not at all easy to determine experimentally the magnitude of the radiosonde thermometer errors at high levels where the errors are more serious. I have, therefore, endeavoured to obtain estimates of these errors by using heat-transfer formulae for the thermometer element treated as a flat plate and the radiation shield treated as a duct. Such variable factors as multiple reflections inside the shield and swinging of the radiosonde have to be allowed for.

The results of these calculations lead, for example, to the following estimates of errors, at 100 mb, for various times of the day in June. They are compared with the corresponding day and night differences given by Dr. Kay.

Hr (GMT)	06	09	12	15	18
Observed excess above night temp. (°C)	2·6	3·2	3·5	3·4	2·6
Calculated errors (°C)	0·9	1·8	1·8	1·8	0·9
Differences (°C)	1·7	1·4	1·7	1·6	1·7

The differences show no indication of regular variation in the daytime.

The rapid increase in the radiation errors as higher levels are reached is due more to the heating of the air inside the shield than to solar radiation falling on the thermometer element. For example for a solar elevation of 40° the calculated errors are as follows :

Height (km)	10	20	30
Error due to radiation on element (°C)	·5	1·5	3
Error due to heating of air inside shield (°C)	·5	2·0	10
Total (°C)	1	3·5	13

Some confirmation of these figures is afforded by a few daytime soundings at 25-30 km in which a sudden fall in temperature of between 10°C to 15°C was observed after the balloon burst and before the parachute opened. In such cases the high rate of ventilation almost eliminates the radiation error.

The estimates I have quoted do not include errors of radiation reaching the thermometer from below, e.g. by emission or reflection from clouds or the earth's surface.

In the new radiosonde under development for the Meteorological Office it is hoped to eliminate radiation errors by using a fine-wire thermometer-element which will need no shield. Comparison of this with the existing type from the same balloon should afford a satisfactory means of determining the errors of the latter; but experience has shown that a fairly large number of such comparisons is required in order to separate systematic from random errors.

Mr. E. Gold : Dr. Kay has shown that the change of temperature recorded by the radiosondes during the hours of darkness is about twice as great as the change calculated by Dr. Gowan. Might this be due to the effect of a change in the low-temperature radiation from the earth falling on the instrument ? About one-quarter of the earth's radiation goes through the atmosphere without absorption and this radiation is appreciably greater at the beginning of night than at the end. The difference is of the order 0·01 g cal cm^{-2} min^{-1} and is possibly enough to affect the temperature of the ventilated instrument by an amount of the order of 0·1°C when in the stratosphere. (For an unventilated black body the temperature change due to a change in the incoming radiation, on one side, of ·01 g cal cm^{-2} min^{-1} would be of the order 1°C.)

If the actual recorded temperature change, corrected by an allowance for this effect, came into close agreement with the calculated value, it would increase appreciably confidence in the other theoretical conclusions based on the application of radiation computations to the upper atmospheres.

Dr. R. H. Kay (*in reply*) : The computations by Dr. Scrase are most important in showing that the large temperature variation measured through the day is quite probably due to thermometer error at these heights. However, there remains the difference in level day to night of about 1·5°K. It may be that a large part of this residual is also due to sunlight. Dr. Scrase will agree that in the quite complicated calculations which he has done there are many variables and also some assumptions which really require an experimental check on the actual sonde. Can it be that the calculated errors are still too small ? From the second table quoted by Dr. Scrase, one would expect the errors at 25 Km to be about 6 or 7°K, somewhat lower than the 10°K experimental figure quoted. However, Dr. Scrase's calculations do emphasize the importance and order of magnitude of radiation errors at these high levels and the need for *experimental* evidence before safe conclusions can be drawn about measured diurnal variations.

As I mentioned in reading my paper, the cosmic-ray physicists have recently been striving to relate diurnal density and height variations at these levels (as published in the *Daily Weather Report*) to diurnal meson formation and decay effects. The underlining of my conclusions by Dr. Scrase should help to prevent further doubtful conclusions being drawn from the radiosonde data in this field.

In reply to Mr. Gold, I would say that the change through the night of order 0·25°K to 0·5°K is probably mainly real, and would not be completely reduced to values nearer 0·1°K required in some radiation calculations by correcting for the effect of earth radiation on the thermometer element. However, it is too dangerous to generalize about such small differences when actual calculation is so complex and, here again, only suitably designed experiments with the sonde in flight can decide Mr. Gold's point with certainty.

551.515.8 : 551.524.7

The modification of continental polar air over Hudson Bay

By F. E. BURBIDGE

(Read 19 December 1951. Presented jointly with paper below by Mr. J. M. Craddock. See *Q.J.*, **77**, p. 365)

551.515.8 : 551.524.4 : 551.521.32

The warming of arctic air masses over the eastern North Atlantic

By J. M. CRADDOCK

(Read 19 December 1951. See *Q.J.*, **77**, p. 355)

Discussion

Commander C. E. N. Frankcom : I welcome the appearance of these two practical papers, both of which have considerable maritime interest. The Hudson Bay area is very important commercially because of the grain port at Churchill and it is interesting to hear these confirmatory reports that the Bay is, in fact, covered with ice during the winter for there has been some controversy about this. It is not surprising, therefore, that the season during which ships can get to Churchill is short, but investigations such as are discussed in this paper may help towards giving advance information as to whether a particular season is going to start early or late as far as shipping is concerned.

Dr. R. S. Scorer : The heat to evaporate water from a wet bulb comes from the air and so heat would flow into or out of a water surface according as the water temperature were below or above the wet-bulb temperature. Thus, surely, Mr. Craddock should relate the heating to the difference between the sea temperature and the ' dirty ' wet-bulb temperature (dirty because the sea is salty). This procedure would of course assume that the heat given by the air to evaporate water from the sea would be regained soon afterwards through condensation and precipitation of the water, but it would enable the latent and sensible heat to be included in one estimation.

Though one naturally thinks of southward-moving cold air masses as subsiding, I am inclined to think that the subsidence is very small or even negative when convection is active and that at a later stage of its journey southwards subsidence begins and convection decreases. Indeed it seems that subsidence and vigorous convection (in the same layer) are incompatible. My experience is that forecasts for the Biscay region in a cold northerly air stream almost invariably overestimate cumulus development because they are based on the behaviour of the same air in the region of latitude 60°N. At 60°N. the air is in the ' ana ' stage; at 47°N. it is in the ' kata ' stage, and in this latter state convection always turns out to be less vigorous than the temperature distribution would suggest.

Dr. R. C. Sutcliffe : I believe studies such as Mr. Craddock has presented to us are of quite fundamental importance in meteorology. While no one would deny that air masses are heated and cooled it is quite commonplace to regard adiabatic change as a first approximation in synoptic dynamics. I believe that it is at best a very poor approximation and Mr. Craddock, and also Mr. Burbidge, have provided valuable evidence.

As an incidental point I would like to ask Mr. Craddock whether he finds it easy to diagnose the upper limit of convection. We have, in upper-air sections, frontal surfaces, subsidence inversions and limits to convection all showing many features in common. I believe it is often very difficult to decide how to classify a given case. Perhaps Mr. Craddock would comment on this.

Mr. E. J. Sumner (*partly communicated*) : I found most of Mr. Craddock's paper reasonably but not utterly convincing, particularly concerning possible dynamical effects. Subsidence and penetrative convection are usually considered to be inconsistent processes, as they manifestly are in most situations. However, in the special circumstances of cold air moving steadily southwards over a warm sea, there is no *a priori* reason why both a general sinking of the air and convection should not go on simultaneously, the ' convectively unstable ' lapse being maintained by the rapidity of the heating from below.

The absence of much ' débris ' cloud in these air masses, or, at least, the speed with which they ' clean up ' inland, suggests the possibility of a certain amount of subsidence. Until the mixing processes are understood in more detail, some doubt will remain.

Mr. J. M. Craddock (*in reply*) : In the first place I wish to associate myself with Cdr. Frankcom 's remarks about Mr. Burbidge's paper. The facts given will assist the medium-range forecaster concerned with the eastern North Atlantic, who has among other things to forecast the thermal distribution over the North American continent as a preliminary to considering the weather in the British Isles.

In reply to Dr. Scorer, a graph showing the flux of sensible heat plus latent heat plotted against the lapse of wet-bulb temperature across the sea surface would closely resemble that showing the flux of sensible heat plotted against the temperature lapse. The association shown in the latter is, however directly applicable in practical upper-air analysis, so I preferred to treat sensible-heat transport and water-vapour transport separately. I think with Dr. Scorer that there is little or no general subsidence during the first southward onrush of a cold air mass, and that the heat gained by the air at this stage is almost entirely derived from the underlying surface. I think that the warming by subsidence commonly associated with such air masses occurs at a later stage, when the circulation which led to the movement of the cold air is declining.

In reply to Dr. Sutcliffe, it was usually fairly easy to identify the convective limit, meaning the level which could be reached but not surpassed by convective cloud forming near the surface; the problem was to sort the cases where the air above the convective limit belonged to the same air mass as that below from cases where the convective limit separated distinct air masses. Looking only at the initial soundings it was often impossible to be sure to which category a case belonged, but a comparison of initial and final soundings usually settled the question. The necessity of doing this led to the drawing of many trajectories which were later discarded, as mentioned in the body of the paper.

In reply to Mr. Sumner, my own opinions as to the parts played in the warming of cold air masses by heat transfer from the underlying surface and by dynamical heating are set out in my reply to Dr. Scorer. The rates of heating found in the present paper are similar to those found when fairly active subsidence is in progress, but there are difficulties in ascribing the warming of these air masses largely to dynamical heating: (1) the association between active convection and vigorous subsidence, though perhaps not impossible, is certainly uncommon, (2) the approximate constancy of equivalent potential temperature with height is a natural outcome of convective activity, but does not naturally follow from dynamical heating, and (3) there is no apparent reason why the intensity of dynamical heating should be related in magnitude to the intensity of the temperature contrast across the sea surface. In view of these difficulties I think that subsidence can play at most only a small part in the heating of these cold, fast-moving air masses passing over warm sea.

Finally, while I think that the evidence presented in this paper raises a strong presumption that the heat gained by these air masses is largely derived from the underlying surface, I do not think it gives indisputable indication of the relative importance of sensible-heat transfer as opposed to latent-heat transfer. Further evidence on this topic could be obtained by studying soundings made at the northern weather ships, which were not in operation during the period covered by this investigation.

551.557.35 : 551.553.5 : 551.508.77

Observations with a directional raingauge

By R. E. LACY

(Read 19 December 1951. See *Q.J.*, **77**, p. 283)

Discussion

Mr. L. C. W. Bonacina : I think Mr. Lacy's line of work is very important in relation to the whole theory of rainfall measurement. To take an extreme case, it is well known that in exposed mountain regions quantities of snow which ought to have fallen on the windward side of a ridge may actually be deposited on the lee side, raising theoretical difficulties as to the side to which the precipitation should be accredited. But it is also known that even rain can to some extent be carried along horizontally in exposed situations, as indeed, countrymen are sometimes heard to remark. When, however, I studied and summarized the early papers in *British Rainfall* to which Mr. Lacy has referred, I learned the important fact that the amount of rain caught on a horizontal surface is independent of the angle of incidence; there is as much with 1° as with 90°. This is no doubt a fortunate simplification of the problem though in exceedingly wind-swept places it may sometimes happen that turbulence prevents the precipitation reaching the ground at all, the angle of incidence being zero or negative.

Mr. J. M. Craddock : Mr. Lacy's suggestion that a wall facing south or west may be damper than one facing north or east agrees with my own experience at Dunstable. I live in the right hand one of a pair of semi-detached houses, facing northwest, built about 12 years ago of rather porous Fletton brick. My neighbour's house, which has the whole area of the side walls exposed to the southwest, is damp in almost every room. My own house is almost free from damp; what there is, being in a back room having a wall facing to the southeast. The houses are, of course, of the same plan and construction and the distribution of damp is what Mr. Lacy's results would suggest.

Asst. Prof. P. A. Sheppard : It occurs to me that Mr. Lacy's problem of the amount of rain which will fall on the windward face of a building may find a rough answer in the computations of Glanert (*A.R.C. Rep. and mem.*, No. 2025, 1940) and later of Langmuir and Blodgett (*U.S.A.A.F. Tech. Rep.*, No. 5418, 1946) on the collection efficiency of a cylinder for cloud droplets. These authors' results are presented in terms of non-dimensional parameters and should be readily applicable to the building problem, except for large raindrops which fall into the airstream flowing round the building only a short distance upwind of the latter.

The fact that aircraft, with a relative airspeed many times greater than the wind at building level, do not in general get their larger parts iced up in a supercooled cloud, suggests that aerodynamic forces may also deflect a high proportion of small raindrops from a windward-facing wall.

Capt. W. N. McClean : I think that accepted experiment has proved that the standard 5-inch gauge, or any larger gauge, gives a reliable value of rainfall, provided that the site and exposure are satisfactory. The speed of the wind determines the slope of rainfall, and consequently gives the rainfall which would strike a vertical wall at right angles to its course.

Dr. J. Glasspoole (*communicated*) : I am very glad to know that the Building Research Station of the Department of Scientific and Industrial Research is taking an interest in the important subject of rainfall in relation to houses and that Mr. Lacy has written a summary of the records.

The normal method of defining the rainfall for different wind directions is from the hourly tabulations of rainfall and of wind, which are available for certain observatories of the Meteorological Office. I hope Mr. Lacy will compare the result from his Garston gauge with those obtained in this manner. Such a comparison would be of value : (a) in showing whether the octagonal gauge sets up local wind eddies which deflect the rain to any appreciable

extent, (b) provide a link for using the hourly tabulations which are available for other parts of the country and (c) enable the wind-force factor, which is available in the hourly tabulations, also to be taken into account.

In this connection reference is made to a paper in the *Quarterly Journal*, **40**, p. 327, 1914, on ' The relation between wind direction and rainfall ' by H. J. Bartlett, which summarizes the hourly values of wind direction and rainfall then available.

Mr. R. E. Lacy (*in reply, partly communicated*) : There appears to have been a considerable amount of interest in the last few years in this matter of driving rain and we have heard of work being done, or about to be started, by Professor Holmgren in Norway, by C. H. Nell in Holland, and at the Central Building Research Institute in India. In addition H. G. Fourcade in South Africa has considered driving rain from the point of view of possible errors in the measurement of rain in hilly country, and a four-sided gauge was built for this study.

Since the paper was written we have made a second gauge and set it up at Thorntonhall (near Glasgow), which is a fairly well-exposed site some 500 ft above sea level. At present the readings of this gauge are being taken daily, and a plot of the computed values of rainfall incident angle against daily mean wind speed shows surprisingly good agreement with the line derived from the Garston observations ($\tan i = -0{\cdot}04 + 0{\cdot}140\, r$), although the scatter is naturally greater than with the monthly readings. It will be necessary to continue these observations for a year or so before this agreement can be confirmed. If it proves that the relationship between rainfall incident angle and wind speed is independent of site, it will he possible to use existing hourly data of rainfall and wind speed and direction to compute the rainfall catch on vertical surfaces in other parts of the country.

It is interesting to note that at Garston, a total of one inch or more of rain has been collected in one or other of the vertical apertures of the gauge in 32 individual months during the 15 years in which the gauge has been exposed, that is in about 2·1 months per year on the average (the extremes were : nil in 1941 and 6 in 1951). In five of these months over 2 in. were collected in individual apertures. At Thorntonhall however, on each of three *days* in December 1951 and January 1952 over one inch of rain has been collected by a vertical aperture; on 8 January the catch was 1·94 in. in the southwest aperture. This gives some idea of the severity of the conditions to which buildings are exposed there.

It has been possible to obtain some confirmation that walls do in fact collect as much water as is indicated by the gauge. Gypsum moisture-gauges have been installed in a north-facing wall of one of the laboratories at Garston, and on 22-23 October 1951 the increase in moisture content of the wall was equivalent to a rainfall of 0·12 in. on the wall face. During this period 0·26 in. of rain, measured by the standard 5-inch gauge, fell on the ground. The mean wind speed during the rain was about 9 mi/hr, veering gradually from west to north. If this wind had been blowing directly from the north all the time it would have been expected (see Fig. 6) that 0·21 in. would have fallen on the wall, which is in fair agreement with the actual catch of 0·12 inch.

I have not yet had an opportunity of studying the papers mentioned by Asst. Prof. Sheppard, but will be interested to see whether some idea of the efficiency of a house, and of the directional raingauge, as collectors of rain can be obtained. If only the very smallest droplets are deflected it is likely that the total mass of water reaching the house will not be greatly reduced. Dr. Glasspoole mentions the hourly tabulations of rainfall and wind available in the Meteorological Office. I hope that, as mentioned above, the work at present in progress will give us the link which will enable us to use these tabulations.

Proceedings at meetings of the Society

Popular Lecture, 5 December 1951

Dr. R. C. Sutcliffe, O.B.E., being in the chair, Mr. J. Paton, F.R.S.E., gave the annual popular lecture on 'The optical properties of the atmosphere,' at the Royal Institution. The lecture was open to sixth-form students, of whom some 300 were present by arrangement with the education authorities of the London, Middlesex and Surrey County Councils.

Ordinary Meeting, 19 December 1951

The following candidates were declared to have been elected that day by the Council :

Fellows

Ashton, Harry Tamblyn, B.Sc., A.Inst.P., 8 The Esplanade, Fairfield, N.20, Victoria, Australia.
Gillman, Capt. Ronald, D.F.C., D.F.M., 4 Tilehouse Way, Denham, Bucks.
Impey, Francis L., Fort Corblets, Alderney, C.I.
Monteith, John Lennox, B.Sc., 111B Grange Road, London, W.5.
Williams, Sgt. Pilot Clive, 167 Holderness Road, Hull.
Wilson, Mary, Mrs., 19 Rack Field, Woolmer Hill, Haslemere, Surrey.

Foreign Members

Ashbel, Professor D., Head of Dept. of Meteorology, Hebrew University, Jerusalem, Israel.
Karapiperis, Photios P., D.Sc., National Observatory of Athens, Greece.
Soontarotok, Lt. Prasert, Meteorological Dept., Royal Thai Navy, Bangkok, Thailand.

Associates

Groot de, Pieter, 66½ Maitland Street, Halifax, N.S., Canada.
Hiles, Donald Wilfred, Radiosonde Station, Churchill, Manitoba, Canada.
Legg, George Herbert, D.A.F.O., Malton Airport, Malton, Ontario, Canada.
Leppard, Ray Emerson, 9345-105 Avenue, Edmonton, Alberta, Canada.
Phillpot, Henry Robert, B.Sc., 20 McLean Avenue, Bentleigh, S.E.14, Victoria, Australia.

The following papers were read and discussed :

(a) 'The modification of continental polar air over Hudson Bay' by F. E. Burbidge (*Q.J.*, **77**, p. 365).

(b) 'The warming of arctic air masses over the eastern North Atlantic' by J. M. Craddock (*Q.J.*, **77**, p. 355).

(c) 'Observations with a directional raingauge' by R. E. Lacy (*Q.J.*, **77**, p. 283).

Ordinary Meeting, 16 January 1952

The following candidates were declared to have been elected that day by the Council :

Fellows

Barratt, Clifford Francis, Section of Meteorological Physics, Graham Road, Highett S.21, Victoria, Australia.

CATLIN, HERBERT WILLIAM, D.F.C., B.A., 4 Russell Way, Higham Ferrars, Northants.
GARNER, RAYMOND JOHN, Officers' Mess, R.A.F., Binbrook, Lincoln.
HUGHES, GEORGE EDWARD GLYNN, 16 Holland Street, Fairfield, Liverpool, 7.
LINEHAN, DONAL L., B.E., B.Sc., 42 North Circular Road, Dublin.
MORIARTY, WILLIAM WARREN, Section of Meteorological Physics, Graham Road, Highett S.21, Victoria, Australia.
RAMSEY, INNES, 6 Murdieston Street, Greenock, Renfrewshire.

FOREIGN MEMBERS

SHERMAN, LEON, PH.D., c/o Department of Meteorology, Florida State University, Tallahassee, Florida.
WEICKMANN, HELMUT, DR. re. nat., RFD. 1, Box 200, Asbury Park, New Jersey, U.S.A.

The following papers were read and discussed :

(a) ' Mountain-gap winds; a study of surface wind at Gibraltar ' by R. S. Scorer (*Q.J.*, **78**, p. 53).

(b) ' The north-east trade of the Pacific Ocean,' by H. Riehl, T. C. Yeh, J. S. Malkus and N. E. La Seur (*Q.J.*, **77**, p. 598).

Discussion Meeting, 20 February 1952

The meeting on 20 February took the form of a discussion on ' Meteorology and the operation of jet aircraft,' and was held at the Institution of Electrical Engineers from 3 p.m. to 7 p.m. The President, Sir Charles Normand, was in the chair, and there were some 250 Members and visitors present. The opening speakers were :

AIR COMMODORE G. SILYN ROBERTS, C.B.E., A.F.C.	' Meteorological phenomena of interest in the design and flying of jet-propelled aircraft.'
CAPTAIN A. M. A. MAJENDIE, M.A.	' Meteorological requirements for the operation of turbo-jet civil airliners.'
GROUP CAPTAIN C. G. LOTT, C.B.E., D.S.O., D.F.C.	' Military jet-aircraft operations and meteorology '.
MR. G. J. W. ODDIE, B.Sc.	' Special problems of forecasting for jet aircraft.'
Mr. C. S. DURST, O.B.E., B.A.	' High-level winds and temperatures for jet-aircraft operations.'

Following a general discussion there was a summing up by Assistant Professor P. A. Sheppard on ' Some deficiencies in current meteorological knowledge in relation to the operation of jet aircraft.' The discussion will be published in full in the July number of the *Journal*.

Reviews

Annual Report of the Director of the Meteorological Office, 1950-51. London (H.M.S.O.). 1951. 8vo. Pp. 53. 1s. 6d.

Those seeking information about Britain's weather service will find in these pages all the facts; official meteorology cost the taxpayer about £2 million in 1950/51 (p. 47); the staff totalled 2,601 on 31 March, 1951 (including 160 Scientific Officers, 613 Experimental Officers and 1,470 Assistants) (p. 35); the Meteorological Research Committee dealt with some 70 technical reports and memoranda (p. 27); the Central Forecasting Office handles nearly half a million message groups every day (p. 10) and so on. The ordinary man or woman, one feels, cannot fail to be impressed by the toil and trouble which go to produce the hors d'oeuvre to the News Bulletin.

It is evident even to the most casual reader of this Report that the Meteorological Office has a heavy double task – to provide a Public Service of a most rigorous and exacting nature, and to be the spearhead of scientific investigations into the nature of atmospheric processes. The supply of forecasts to the general public, government departments and other undertakings demands of itself the undivided attention of a large body of highly trained and hard-working scientific personnel. To these must be added the provision of specialized information to the Royal Air Force, civil aviation, the Navy, the Army, Ministry of Supply, Merchant Navy and fishing fleets, the Commonwealth and foreign countries. On a longer time scale, climatology occupies nearly 7 pages of the Report with a mere recital of activities, and even so, one notes such items as " owing to lack of staff no further progress was made with the volumes [of the *Réseau Mondial*] for 1933-39." It is pleasant to see that upper-air climatology is receiving all the attention that can be given, and no doubt some day we may hope for news that a climatological summary of the upper atmosphere is ' in the press.'

These are ' routine ' matters; meteorology is now part of the life of a civilized community and it is for these services that the taxpayer parts (one hopes cheerfully) with his £2 million. Even so, it comes as something of a shock to those who, like the present reviewer, are no longer part of the organization, to realise how much is demanded of the relatively small staff, and one looks with new respect at the hard-working professional meteorologist, who must not only keep the machine running smoothly and to time, but is also expected to contribute to the advancement of the science as a whole. To the readers of the *Journal* the most interesting part of the Report is doubtless that which deals with research, but it should not be forgotten that the Director, however much he may cast longing glances at promising fields of inquiry which lie all around him, is under a statutory obligation to maintain an efficient public service, and the number of men he can spare for full-time research must necessarily be very limited.

Despite these conflicting demands, it is evident that a great deal of genuine research is going on in the Meteorological Office; some eight pages of the Report are devoted to this matter. During the anxious days of the War, the present Director had the courage and foresight to form his Meteorological Research Committee, and has kept it going ever since. There is now a Deputy Director in charge of research with responsibilities not only for General Research and Observatories and Special Investigations, but also for Instrument Development and Provisioning, Climatology and Agriculture and Marine Meteorology. The Meteorological Research Committee, under Professor G. M. B. Dobson, consists of seven independent and five ex-officio members. It has three sub-committees, dealing with instruments, synoptic and dynamical research and physical research, respectively, each with an independent chairman and many specialist members. The main items on which attention is now being focused are as follows :

(i) development of aircraft thermometers and hygrometers, especially for high-speed machines; (ii) investigations into upper-air turbulence; (iii) measurement of upper winds by radar; (iv) investigations into the physical structure of rain-producing clouds; (v) ice accretion on aircraft; (vi) radiation fog; (vii) improvements in short-range forecasting; (viii) extensions of weather forecasts to cover three or four days; (ix) temperatures and humidity conditions in the troposphere and lower stratosphere; (x) investigations into tropopause conditions and air exchange between the troposphere and stratosphere.

Anyone with a knowledge of practical meteorology will recognize in this list some familiar and formidable problems, and will not expect rapid progress towards their solution. What the Report has to say is encouraging. Much of the instrumental work is now at an advanced stage, and a good deal more is known about the microphysics of clouds than hitherto. There has been much valuable work on radiation and heat exchange near the ground. The hardest nut of all – the basic synoptic problem of the development and movement of depressions and anticyclones – has been attacked in various ways, chiefly by methods of computation based on the fundamental dynamical equations. As befits the importance of this project, the Report is cautious and claims only ' some promise,' coupled with the warning statement that, so far, the mathematician has not drawn ahead of the experienced forecaster, who presumably relies chiefly on a flair for his job, developed by long acquaintance with weather maps and with the vagaries of British climate in general.

It becomes evident, however, that with all the goodwill in the world, the Meteorological Office is not finding it an easy task to maintain research at the level required for significant advances. There are many hints of this in the taut official phraseology – a larger research staff than had been allowed for initially would be an advantage (p. 26); there are difficulties in recruiting Scientific Officers qualified

to undertake research in meteorology (p. 27) and, in general, the officer strength is insufficient to mee requirements (p. 35). Some, at least, of these difficulties can be traced to the indifference of the universitie of this country to meteorology, which has never had the whole-hearted support which astronomy, fo example, has always received from the academic worker. It may be that classical physics is now unfashionable for research, (and meteorology is mainly a matter of classical physics and mathematics); and that the young scientist yearns to make his mark in more recently opened fields, but whatever the cause, it is a matter of concern that the country which produced so many pioneers of meteorology should be apathetic to the need for basic work in the science of the atmosphere. One of the chief functions o the Meteorological Office is to test, develop and apply the theories and suggestions of the more academi worker, who alone has the opportunity to ponder on the foundations of the science in something approaching tranquillity. One has only to compare the support which the defence-research establishments for example, receive from the universities in nuclear physics, electronics and aerodynamics, with the small amount of attention given to atmospheric problems outside the Meteorological Office, to realise how much could be done to help to place one of the most difficult branches of natural science on a more secure footing.

Official statements and Blue Books rarely make entertaining reading; their business is to be factua and objective, and one would not have it otherwise. To the general public the Meteorological Office is an organization for producing the daily forecast and, like the Civil Service in general, a dearly-lovec source of jokes for music-hall comics. Probably only a very small fraction of the population has any conception of the real extent of the work of the official organization, and for this reason it behoves anyone who takes an interest in meteorology, either as a science or as an aid to better living, to read through this Report and to ponder a little on the breadth and depth of the activities of the Meteorological Office There is much food for thought here.

O.G.S.

Linkes Meteorologisches Taschenbuch. Vol. 1 (New Edition). Ed. F. Baur, Leipzig (Geest and Portig). 1951. 8vo. Pp. 360; Tables. No price given.

Smithsonian Meteorological Tables. Ed. R. J. List. (Sixth revised edition). Washington (The Smithsonian Institution). 1951. 8vo. Pp. 527; Tables. No price given.

These two compilations need no introduction to most readers of this Journal.

Professor Franz Linke died in a heavy air raid on Frankfurt in March, 1944. As Dr. Franz Baur says in the foreword to the new edition of the first volume of Linke's well-known reference book, this work endures as the best memorial to this distinguished meteorologist. In this volume are collected up-to-date information on the codes, broadcasting and plotting of synoptic messages, and lists of reporting-station numbers (all assembled by Karl Keil). It also contains the dictionary of meteorological terms (in German, English, French, Italian, Spanish and Russian) prepared by Bergeron; details of the structure of the I.M.O. (not the W.M.O.); and a revised summary of monthly and yearly averages of pressure (in mb, reduced to sea level) and temperature (in °C) for a world-wide network of stations, which has been drawn up by Karl Knoch and Alfred Schulze. The structure of the decimal-classification of meteorological literature is detailed, and of rather less value are a short section on the International Cloud Classification and the recognised abbreviations of cloud names, and a somewhat expanded list of historical meteorological events (which extends up to 1941 but contains only 4 entries since 1921).

It would of course be quite impracticable to detail the contents of the Smithsonian tables. A first inspection suggests that the latest edition includes everything of conceivable value, all assembled with the utmost care. The value of such a book is apparent only on the occasions when its help is sought; however rare these occasions might be its absence from the library shelf would be as distressing as a missing front tooth, and we must all be grateful to those who have devoted so much time and care to its preparation.

F.H.L.

Dynamische Meteorologie (*Physik der Atmosphäre*, vol. 2). By H. Koschimeder. Leipzig (Geest and Portig). 1951. 8vo. 3rd revised edition. Pp. x, 404 ; 6 tables. No price given.

The first edition of Koschmieder's *Dynamic Meteorology* appeared in 1933 when the textual literature on the subject was not very extensive, in German, or any other language. Today the position is very different, particularly for the English reader, who is now rather unlikely to go to a German text for his instruction unless it possesses marked virtues not to be found elsewhere. Consequently our remarks on this revised, third edition of Koschmieder will be made against that state of affairs.

What then are the special virtues of Koschmieder, 1951? First, an orderly development of the ubject in which the author appears to be at pains to relate the physics as closely as possible to the mathe- natics at every stage. This is instanced by the liberal use of well-conceived diagrams and the presence f many tables of numerical matter to illustrate the equations. The book is in three parts: statics, kine- natics and dynamics, of which the first and third are the largest, but the second most original in treatment. A price must be paid for the separation of kinematics from dynamics but there is a logic in the approach vhich has much to commend it, particularly for the reader strange to hydrodynamics. It is interesting o notice in the kinematics a fuller treatment of the classical work of Shaw and Lempfert on air trajectories nan is available elsewhere in the textual literature.

Most of the first part is devoted to quasi-static motion, and the introduction and use of the Second Law are notably well-treated, as are also Normand's and Littwin's contributions to the subject.

The dynamics is oddly patchy and ill-balanced. Thus there is hardly a mention of the cyclone, nticyclone or other major pattern of atmospheric flow to be found in this part and there is no reference t all to the general circulation. But it does contain a quite full treatment of Bartels's tidal theory, and f Lyra's work (14 pages) on mountain waves. Kleinschmidt's analysis of dynamical instability is also iven. The most successful chapters of this section are on energy-transformations, which are more ully discussed than usual, in particular Sandström's theorem on the location of heat sources and sinks n the atmosphere.

It is evident that this text will not replace others from the shelves of the English-speaking meteor- logist. But it could well find a place in his library for the topics which are largely or quite untreated lsewhere and are well treated here. The reviewer, no judge of German style, found the writing particu- arly clear and simple in construction.

P.A.S.

Recent additions to the Society's Library

Annual Report of the National Oceanographic Council 1950-51. Cambridge University Press, 1952. Pp. 61, charts only. 5s.

Commonwealth of Australia, Meteorological Branch, *Book of normals, No.* 1, *Rainfall . . .* 1911-1940. N. d. Pp. 169, map. N.p.

D. A. Davis, D. Hepburn and H. W. Sansom, *Report on experiments at Kangwa on artificial stimulation of rain January-April* 1951. East Africa Meteorological Department, 1951. Pp. 31, diagrams, maps. 5s.

W. J. Gibbs, A. V. Gotley and A. R. Martin, *A.N.A.R.E. Reports, Series D. Meteorology, Heard and Macquarie Islands*, 1948. Vol. 1. Part 1(b), Analysed Charts. Melbourne, Antarctic Division, Department of External Affairs, 1951. Pp. 61, charts only. N.p.

Istituto Talassografico Misc. Publ., Nos. 272 to 276, No. 273 being 'One hundred years' observations at Trieste (1841-1940); No. 273, part 6, 'The squalliness of the wind.' Trieste, 1950-1. N.p.

P. P. Karapiperis, 'The climate of Blue Hill according to air masses and winds.' *Harvard Meteorological Studies*, No. 9. Cambridge, Mass., Harvard University Press, 1951. Pp. 105, plate, Figs, tables. 8s.

Meteorological Office, *British rainfall* 1949. London, H.M.S.O., 1951. Pp. 212, plate, maps. 25s.

T. A. Ryder, *Weather whys.* Kingswood, Surrey, A. G. Elliot, 1952. Pp. 143, plates, Figs, 6s.

J. R. Scott, 'On the development of horizontal divergence and vorticity in the low latitude atmosphere.' *Memoirs of the Malayan Service*, No. 5. London, C. F. Hodgson. N. d. Pp. 20, Figs. N.p.

The following additional journals are now being received on an exchange basis: *Arkiv för Geofysik*, Stockholm; *Memoirs East African Meteorological Department*; *Meteorologische Rundschau*, Bad Kissingen.

Obituary Notices

Captain T. R. DOW

Captain Thomas Ronald Dow, who had been a Fellow of the Society since 15 January 1930, died suddenly on 9 December 1951 at the age of 51. A native of Kirkcaldy, Fifeshire, Dow was educated at the High School there and in 1916 became apprenticed to the Bombay Persia Steam Navigation Company, afterwards known as the Mogul Line. All the rest of his life was passed in their service. He obtained his Master's Certificate in 1923 and five years later received the first of his appointments in command. In 1937 he was promoted to the post of Marine Superintendent, which he continued to hold until the time of his death.

E.L.H.

Colonel H. W. MIREHOUSE, M.B.E.

Colonel Henry William Mirehouse, of Ruan Minor, Cornwall, whose death occurred on 3 January 1952, was one of the Society's many Fellows who developed an interest in meteorology during their period of service with the armed forces and made a special hobby of the science after their retirement. Mirehouse joined the Honourable Artillery Company in 1914 and on the outbreak of war was granted a commission in the South Lancashire Regiment. Not long afterwards he received the award of the M.B.E. for gallantry in the field. His subsequent military career took him, via the Staff College at Camberley, to various staff appointments at home and overseas. From 1938 to 1940 he was Assistant Adjutant and Quartermaster General at Force Headquarters, Palestine; from 1940 to 1944 he filled with distinction a post as Colonel i/c Administration in Egypt. Ill health compelled him to relinquish his duties in 1945, and in the following year he was invalided out of the Army. Since 1947, when he took up residence in Cornwall, much of his time had been given to observational meteorology at his home near Helston. Colonel Mirehouse was elected to Fellowship of this Society on 21 December 1949. In the autumn of 1950 he attended the pioneer vacation course in meteorology held at Malham Tarn, entering wholeheartedly into the spirit of this first venture.

E.L.H.

The Reverend Canon E. F. ROBSON

We have to record the death on 19 February 1952, at the age of 67, of Canon Eric Fairfax Robson, Vicar of St. Andrew's, West Kirby, and a Fellow of the Society since 1924. Educated at Birkenhead School, Emmanuel College, Cambridge, and St. Aidan's College, Birkenhead, he served two curacies in the Wirral before going to West Kirby in 1917 as curate-in-charge. He was appointed the first vicar of the parish in 1920 and remained there for the rest of his life. To within an hour of his decease he was busy with his parochial duties. Canon Robson maintained a climatological station, data from which have appeared in the *Monthly Weather Report* of the Meteorological Office since 1926; his observations were noted for their attention to detail. He was well known in the north-west as a frequent contributor to the local press of articles on a variety of meteorological topics. He held Fellowship in the Geological Society also.

S.E.A.

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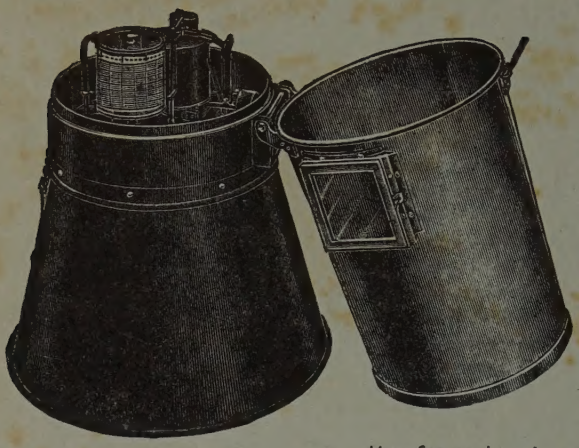

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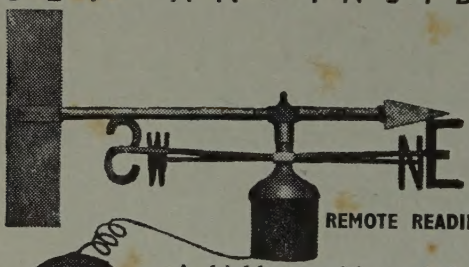

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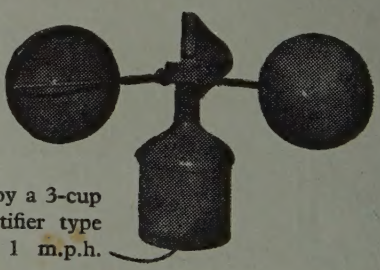

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